HEAD'S CONCEPT OF THE SCHEMA AND ITS APPLICATION IN CONTEMPORARY BRITISH PSYCHOLOGY

PART IV. WOLTERS' THEORY OF THINKING

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I. WOLTERS' THEORY OF THINKING

Wolters(4) has applied the concept of the *schema* in a preliminary attempt to elucidate the nature of *thinking*. He has also made suggestions in a later paper(5) to the effect that the "...conception will cover all parts of animal and human psychology, pulling together into a system many heterogeneous results". In the same paper he has indicated briefly the manner in which the concept might find applications in social psychology.

(1) *The logical and the psychological approaches to the problem of thinking*

Wolters is concerned principally with thinking in its *conceptual* aspect, and sets out from the position that, whatever may be the proper definitions of the terms *conceptual* and *universal* (that is, the adjective 'universal'), there is some valid sense in which we may be said to think 'universally'. But, he maintains, it is necessary in considering psychologically the problems raised by universal thinking to avoid the prejudices introduced by treatment of the subject along lines laid down by the *logical* essentials of the process. In particular we must not be bound by the notion that the concept is a mental entity, nor should we assume that concepts must be involved in the thinking process in the sense that they are *objects* of thought directly present to the mind of the thinker. Putting aside, then, lines of approach that rest upon such assumptions, and in particular that which calls for a phenomenological examination of concepts as the essential objects of thought, he proposes to consider the question of 'how we think', as opposed to that of 'what we think'.

The adoption of such an attitude in the theoretical analysis of thinking is evidently somewhat novel. The escape from traditional concept analysis has too often been effected only through a wholesale denial of the universal nature of thought, and has entailed the acceptance of an empiricist interpretation incapable of constructing any picture of the thought process that will bear serious comparison with its original. Wolters is concerned to insist upon the manifestly universal nature of thinking, without accepting implications which, psychologically, are gratuitous.

(2) *Thinking as a form of behaviour*

As a first step towards answering the question 'how do we think?' Wolters lays it down as obvious that "...thinking is a reaction to a set of circumstances which demand a response by the organism, though the response need not issue in muscular activity" ((4), p. 134). Thinking is thus, in a sense that is reasonable (though not identical with that of the Behaviourists), a form of behaviour. And it is important, he insists, that

conventional classifications of modes of behaviour, such as that into 'theoretical' and 'practical', shall not be allowed to force any a priori characterization upon thinking so considered. Accordingly, instead of fitting thinking into the general category of behaviour upon a basis of characteristics conventionally ascribed to the process, he considers the inverse question of how far its *universal* quality is manifested in other varieties of behaviour. He has little difficulty in showing that, in the simple, immediate sense in which thinking is universal, other varieties of behaviour equally involve concepts.

These other modes of behaviour are ones which, in the first place, we should not suggest involved thinking in the ordinary sense, and in the second would not appear to be susceptible of reasonable elucidation by any assumption that the organism has before it a concept *qua* mental entity. Such is the case of the dog, which "...recognizes any dog as a dog and any cat as a cat, as is clear from the appropriate direction of activities towards the members of the two species" ((4), p. 135). And, he continues, in such a case the human being is little better off. Appropriate behaviour towards a dog, behaviour that is certainly universal in quality, is not mediated by the utilization of the concept 'dog' as a mental entity. All that is, or—it would seem from experience—can be present to the mind is (*a*) the word 'dog', (*b*) (possibly) a dictionary definition of the word, whose usefulness in promoting appropriate behaviour is insignificant, and (*c*) "...a sense that I know something of dogs and can deal with them, but it is impossible to state what it is that I know" ((4), p. 135).

(3) *The adaptive significance of universal behaviour*

Such universal behaviour has, both in man and animal, certain important characteristics connected with its adaptive significance. Its essence consists on the one hand in the capacity for dealing selectively with certain kinds of situation, so that the organism has at its disposal appropriate anticipatory patterns of behaviour differentiated in accordance with the environmental differentiation which is significant for its needs. On the other hand, there is no rigid predetermination of *actual* behaviour, such as is found in fixed habit. Thus, while the organism is saved from purely random trial-and-error behaviour on the one hand, on the other, it is no slave to habit. But it is important to notice that, for Wolters, such behaviour is *not* to be *defined* merely by reference to its position between these two extremes. The universality of behaviour is not mere freedom from too rigid determination. In so far as the universal is a factor determining behaviour, the latter is not indefinite, but flexible. Nor do the specificity and preparedness merely reflect a process of habit formation cut short and maintained in incompleteness for reasons of adaptiveness. The actual behaviour adopted by the organism on any particular occasion is thus determined, on the one hand, by pre-existing tendencies of a generalized nature, and this is the significance of the behavioural appropriateness which in linguistic description we should characterize by the aid of concepts. On the other hand, the concrete behaviour is determined in detail by the details of the stimulus situation, and may be mediated by more or less rigid patterns of habit, or by random trial and error, according to circumstances.

(4) *The schema and universal behaviour*

This universal aspect of behaviour, whether it involve thinking or not, can, according to Wolters, most profitably be approached through Bartlett's concept of the *schema*. Wolters does not explicitly state what his view of this conception may be, and, as we have already seen, there are a number of aspects of it, not all of them mutually consistent.

But from the general context, it is permissible to suppose that, broadly speaking, he is referring to the *schema* as something embodying the following features: (*a*) It is a system of generalized *tendencies*, the product of past experience so organized that there is assimilation *not* by similarity, contiguity, etc., but by those relationships that are of *direct* adaptive significance. (*b*) It is an entity[1] which functions in determining present behaviour, *not* by mere additive co-operation with other factors, but by providing a basic regulative framework within which these other factors, such as the present stimulus, the existence of specialized habits, or trial-and-error behaviour, may function. (*c*) It is something which may function more or less autonomously, that is without the mediation of conscious states directed upon the situation.

It is evident that some such postulation would serve as a basis for the interpretation of those aspects of behaviour which Wolters has in mind, and such a conception, although somewhat different from Bartlett's general notion, is at least consistent with some parts of it.

(5) *The concept as schema*

The next step in the argument is to *identify* the schema with the universal or concept. Since at the level of unreflective, but universal behaviour, the concept manifests itself only in the fact of schematic determination, it is psychologically legitimate, Wolters holds, to maintain that the concept *is* the schema. "...If concepts can be recognized at all in such activities they can only be found *in* the pattern of the behaviour....Consequently, if we are to admit the term 'concept' at this stage it must be applied to that flexible organization of reaction tendencies which provides a schematic preparation for various activities directed to the satisfaction of a recurring need. This may be expressed more briefly in popular terms by saying that the concept consists in 'knowing what to do about it'"[2] ((4), p. 137).

Wolters now seeks to apply this notion of the concept to the problem presented by *abstract* thinking. At this level, we are concerned with concepts as the objects of thought, not merely as the instruments by which we think. Thus, in performing the ordinary concrete operations of arithmetic, the concept of *number* is only *involved*, and, according to Wolters, involved in the sense that behaviour is determined by the operations of the 'number schema'. But if, instead, thinking be directed towards the problem of the nature of number in the abstract, then the concept which permitted simple arithmetical operations to be performed is now itself the object of thought. And thus, according to his contention, the *schema* has become the object of thought, that *towards* which the organism reacts. The questions thus arise, first, of the manner in which schemata, as organized systems of reaction tendencies, can become the objects upon which reactions are directed. Secondly, there is the problem of the nature of the process when this reversal of role has taken place.

(6) *The 'turning-round' process and abstract thinking*

In so far as the first of these problems is concerned, Wolters adopts in essence Bartlett's conception of the organism "turning round upon its own schemata, and organizing them

[1] The word *entity* is used here for purposes of explanatory convenience. Wolters differs from Bartlett, in saying that "...probably we should conceive them [*i.e. the schemata*] as existing continuously, *as characters of the organism, but not as entities which the organism 'possesses'*" ((4), p. 136. Italics ours). This point of difference possesses more than verbal significance, as will appear later.

[2] This line of thinking has been developed by one of us (R. C. O.) in a discussion of the psychology of judgement in general, and the assessment of personal qualities in particular (see Oldfield (3), pp. 42–6, 112–16). It would seem capable of an even wider application in these, and related, fields.

afresh". Bartlett, as we have seen, identifies this act with the essential function of consciousness, and invokes it to mediate behaviour in circumstances in which schematic determination breaks down. In much the same way, Wolters points out that abstract thought affords instances of the breakdown of schematic determination that are peculiar, apparently, to the conscious life of human beings, and that in an important sense reconstruction of the schemata is the result. In the measure in which we think about concepts, those concepts and their interrelations are altered. Thus in thinking abstractly we are, essentially, seeking to reorganize our own systems of reaction tendencies.

But Wolters differs from Bartlett in the precise terms in which he formulates the 'turning-round' act. As we have already seen, for Wolters, the schema is no entity 'possessed' by the organism, but an enduring character of it. Accordingly, he refers not to the organism turning round upon its schemata, but to the organism as *turning round upon, or reacting to, itself as schematized* ((4), p. 140).

In abstract thinking, then, the organism reacts to itself, or, more precisely, to a particular aspect of itself. And the question must arise of the kind of reaction which takes place. Evidently it cannot be of a purely schematic nature, and, in point of fact, it "...often proceeds by blundering trial-and-error efforts which are largely experimental in character" ((4), p. 141). It is not, on the other hand, devoid of schematic determination, but such schemata as are available will not suffice to determine successful reaction. Thus, in thinking about the nature of *number* we should utilize other concepts, such as those of 'quantity', 'individual', 'class' and so forth, and in the measure in which they are utilized, the schemata which are these concepts will determine our reaction.

Wolters thus provides, not perhaps a theory or explanation of the process of thinking, but a reformulation of its essential problem. The object of thought is not the concept in the conventional sense of that word—a mental entity owing its existence and nature less to psychological fact than to a logical conjuring trick—but, less mysteriously, to the organism itself. The detailed problems of thinking are those of the detailed means by which this reaction of the organism to itself is carried out. And these problems are thus left intact for treatment by any method, phenomenological or other, which may be appropriate.

II. Comparison of Wolters' and Bartlett's theories

In thus utilizing, and emending, Bartlett's conception of the 'turning-round' process, Wolters has, on the one hand, we believe, applied it in a manner not ultimately consistent with Bartlett's usage, but, on the other, has presented a picture of the process which is in some respects more comprehensible than that of Bartlett. We have seen that superficially the two conceptions are similar in that the 'turning-round' process is associated with the breakdown of pure schematic determination. They are similar, further, in that it involves, characteristically, conscious function, and that it results in the reconstruction or reorganization of schemata. But in discussing the process, Bartlett is employing the notion of the schema primarily in a somewhat specialized sense, that of organized material. And the result of the process is essentially a reorganization of the material in a form more suited to the determination of adaptive behaviour in the organism. In particular, one special mode of reorganization is that by which the organism is enabled to escape from too rigid determination by the chronological features of schematic organization. But for Wolters, the schema is no organized mass of material. And reaction to it involves no process of chronological rearrangement. The schema is for him a system

of tendencies, and has no chronological structure. Again, for Bartlett, the end-product of the 'turning-round' process is the reaction of the organism to a concrete situation, specifically, one in which the recall of past events is being demanded. For Wolters, on the other hand, it is a reorganization of tendencies which though not in immediate operation, may or may not become effective at a later date. In these, and in other respects, there are divergences of view which are not without significance. The mere circumstance that, for Bartlett the 'turning-round' process provides the organism with concrete facts, whereas for Wolters it may, and frequently does, provide abstract propositions, serves to indicate that their respective notions of the *schema* must differ in some fundamental yet not easily definable manner.

We have already pointed out some of the difficulties inseparable from Bartlett's conception of the 'turning-round' process. On Wolters' view, the process is somewhat more understandable. Since schemata are for him not entities possessed by the organism, but characteristics of the organism, the latter must be regarded as turning round upon itself *as schematized, and the uniqueness of the process consists only in the uniqueness of the object of reaction, and not in the nature of the reaction itself.* Thus, in the same way, we may react to people by talking to them, and if instead of talking to other people we talk to ourselves, this act is unique only in the fact that it is ourselves to whom we are talking. For Bartlett, on the other hand, the very act of 'turning-round' is in some way novel and unique, distinct in kind from other modes of behaviour. In thus bringing the process into line with other types of activity, and in avoiding the logical difficulty inherent in the notion of an organism constituted of schemata turning round upon a part of itself, we believe Wolters to have made a notable advance in the application of the notion. On the other hand, it must be admitted that grave difficulties still remain in the idea of an organism attempting to reorganize its own reaction-tendencies, quite independently of whether or no this process constitutes abstract thinking.[1]

III. CONCLUSION

We have tried to follow a line of thought which, although it possesses unmistakable continuity, is at times tortuous, indistinct and ill-drawn. These are faults inseparable from a stern refusal to pursue the paths of convention where they skirt around essential difficulty. But to excuse such faults demands an answer to the question: "In what respect is the theory which we have considered significantly novel?" Novelty alone is the empty achievement of all departure from tradition. But significance requires improvement upon it. It implies some positive addition either to established knowledge, or to the conditions which render progress possible. It is in the second rather than the

[1] In a discussion of this paper, Prof. Bartlett has objected that there is a difference between the notions of an organism "reorganizing its own reaction tendencies" and of its "turning round on itself as schematized". In the first place, he believes that tendencies are not introspectible, and cannot therefore be made objects of reaction. And in the second place, he claims that in recall attention is directed on *what* was reacted to previously and not upon *how* the reaction was made. These points raise again the whole question of Bartlett's treatment of psychological material and his views on the relation of material to function. We have already pointed out in Part III of this essay (*Brit. J. Psychol.* XXXIII, 123, footnote 3) that Bartlett, in conversation at least, renounces the 'turning-round' conception in the form in which it was put forward in *Remembering*(1). He now prefers to speak of the process in terms of reorganization of the schemata themselves. The latter, moreover, no longer have the character of organized material, though their activity may *produce* such material (*Brit. J. Psychol.* XXXIII, 118, footnote). This material may then become the *object* of other reaction tendencies which elaborate it in various ways. The 'turning-round' process, otherwise recall, thus resolves itself into an influence of certain reaction tendencies on *material* produced by schematic activity. For Wolters, on the other hand, the 'turning-round' process remains one of reorganization of the tendencies, or *modes* of reaction, themselves. The debate continues.

first of these respects that we believe the concept of the schema to form a significantly novel contribution to psychological theory. Its true value lies less in novelty of content than in freshness of approach—less in positive achievement than in brilliant suggestion.

Among those psychological problems which ignorance rather than understanding allows us to formulate in a single sentence, none appears more central than that of the means whereby past experience finds its place in present behaviour. Deeply engrained in most of the attempts which have hitherto been made to elucidate this problem is the tendency to adopt without question the general hypothesis that the process must include at least the following three stages: first, the making of a record, directly or indirectly representative of the experience; secondly, its storage, without essential change in representational character; third, its ultimate revival—revival in the sense that its causal efficacy in determining behaviour is a function of its representational character. This hypothesis emerges in various concrete forms, and upon its precise form depend the special problems that it raises, and the special advantages that are claimed for it. It is a hypothesis which may indeed be true, but one which we believe to be at present wholly gratuitous.

The theory of schemata, on the other hand, demands that we consider an alternative possibility. This is that a given experience, *already in being experienced*, plays the whole part which it is destined to play in determining the future behaviour of the organism. This moment over, it lives only in the modification it has brought about in that organized system, called the *schema*, which is the immediate determinant of response. And this modification is not to be conceived as in any sense a pictorial representation of what has occurred, but rather, to replace one useful crudity by another, as an improvement in a piece of mechanism.

Such a view evidently demands that we account for a capacity which we all, with some justification, believe ourselves to possess, namely that of recalling particular past experiences in a circumstantial manner. Bartlett's answer is to assert that, *psychologically*, the processes by which recall of the past is accomplished are precisely those which we use in anticipation of the future. Accurate recall and accurate prediction are alike the positive achievement of a constructive process, a view which most people certainly hold about prediction at least.[1]

It may be, and we believe, that the concept of the schema will ultimately prove unprofitable in the form in which its authors have left it. But it will not have been devised in vain if it serves to disclose unwitting presupposition, and thereby to allow fairer play to those powers of constructive thinking, which, no less than apt experiment, are indispensable to the development of psychology.

We should like to express our special thanks to Prof. Bartlett, who so gladly has given time and patience to the unravelling of many of our perplexities. To him alone the inspiration of this work is due. We have also to thank Mr A. W. P. Wolters, who, in conversation, has amplified for our enlightenment his all too brief writings. Among others who have read this essay and helped us by their criticism, we wish especially to thank Prof. G. Humphrey, Dr L. M. Hurvich and the late Prof. K. Koffka. Lastly, our friendly gratitude is due to Dr Mary Northway who, in anticipating our task, has so ably compensated for many deficiencies in its execution.

[1] Some of the relations between recall, imagination and thinking have already been indicated in *Remembering* ((1), pp. 205, 311–14) and Prof. Bartlett has recently been undertaking research on constructive thinking itself along these lines. Some of the earliest results of this work have been made available in a preliminary paper (2).

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SOME EFFECTS OF VEILING GLARE IN BINOCULAR VISION

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I. Introduction

Martin & Pickford have described an experiment dealing with the effects of veiling glare upon phenomenal regression for size in monocular vision(1, 2). The results were compared with binocular phenomenal regression under conditions similar to those in the monocular measurements, but it was not possible to use the veiling glare in binocular vision. It seemed important to set up an apparatus with which veiling glare could be used when vision was binocular, because most ordinary vision employs both eyes. It was also important to consider the problem of apparent distance as well as that of apparent size.

Warner Brown gives 'aerial perspective' as the fourth condition of distance perception (see (3), pp. 218–19). This is a well-known condition, and seems to depend on the reduction of distinctness owing to the haze or dust in the air between an object and the observer. Objects seen through haze may look farther away than when clear, and most observers have noticed that haziness influences apparent size as well.

Babington Smith & Pickford have reported a short experiment in which a binocular apparatus for veiling glare was used(4), and the present writer now wishes to restate the results of that experiment in a fuller form, and to consider in addition another experiment which was carried out later with the same apparatus. The apparatus will be described first and then the purpose and technique of each experiment will be dealt with separately. Two control experiments will also be described.

II. Apparatus

A box blackened inside was divided into a left- and a right-hand section by a milk glass panel. Eyeholes were cut in one wall of the left-hand section, and a head-rest taken from a Brewster stereoscope without the prisms was fitted. An opening in the opposite wall of the box gave the subject an unbroken field of vision about 1 by 2 m. at 3 m. distance. In the right-hand section of the box were fitted two 60 W. lamps, side by side. They could be switched on either separately or together. Light from either of them, passing the milk glass panel, was reflected back into the subject's eyes from a piece of plate glass set across the left-hand section of the box at 45° to the central line of vision. The proportions of the whole box and the distance apart of the lamps were so calculated that either lamp would produce a veiling glare about twice as great on one side of the visual field as on the other. Thus the subject, looking through the eyeholes, saw a veiling glare graded from left to right or from right to left. If the front lamp was used, the glare decreased from right to left, and if the back lamp was used it decreased from left to right. The subject could see two circular white disks 11 cm. in diameter, which stood on a blackened table, and were about 60 cm. apart when 3 m. from his eyes. Each disk was fitted to a circular black card, about 20 cm. in diameter, and the card and its stand were invisible against a black background. The table was also invisible to the subject.

The disks could be moved by the subject towards or away from him, by means of blackened strings, and since they were set to move in lines radiating from the point between his eyes, they could be varied in apparent size and distance, but not in apparent distance apart, though the nearer disk tended to look slightly higher than the other because it subtended a slightly greater visual angle. Thus the distance cues were cut down to a minimum, which was required in an experiment whose conditions were intended to be comparable with those of fog vision. The disks were illuminated by a 40 W. lamp in the centre of the room, above the space between the subject and the table. Either disk could be used as a standard, at 3 m. distance, and the subject could adjust the other to

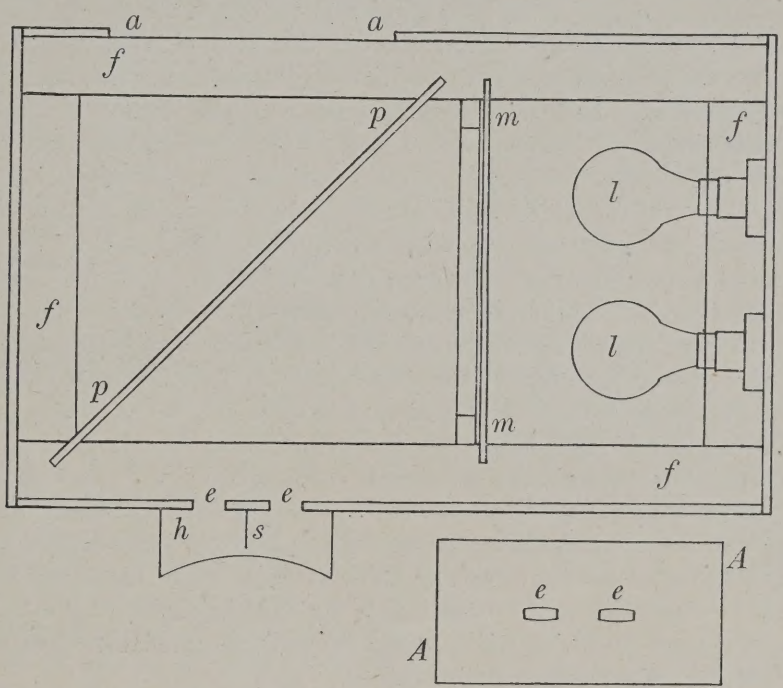

Fig. 1. *l*, lamps; *m*, milk glass; *p*, plate glass; *f*, frame of box; *a*, front aperture; *A*, front aperture in elevation showing eyeholes as if in same plane; *e*, eyeholes; *h*, headrest; *s*, screen to prevent light from *m* passing into left eye via right eyehole. (Scale 1/6).

make it appear equal in size or distance. The apparatus is shown in Fig. 1. With this apparatus there is a slight binocular difference in the intensity of the differential glare, owing to the distance apart of the two eyes, but I think that it may be neglected. It reverses when the haze difference is reversed.

III. SIZE AND DISTANCE METHODS

Either disk could be the standard; the greater glare could be on the right or the left; the variable disk could be set initially too far away or too near. In Exp. 1 the eight possible combinations of these conditions were taken in turn, with nine subjects, and an equal number of settings of the variable was taken for each combination, the subject making the adjustments, after the manner of the classical experiments by the Method of Production(5). The primary problem was to measure the average difference in apparent distance produced by the differential veiling glare.

The Production Method may seem to be the simplest and most efficient for such an experiment when it is practicable. It appeals to subjects, who frequently assume that, if they are asked to estimate or to observe equality between two stimuli or objects, they should be allowed to manipulate the objects themselves. It also appeals to inexperienced experimenters. This method was used successfully in a previously reported experiment(2), but it has been abandoned for future experiments on veiling glare. The Constant Method and the Limiting Method have both been tried, and the latter will be adopted in future. The Constant Method, in spite of its appearance of being more scientifically adequate, is cumbrous, confusing to the subject and unnatural. The Method of Production, in spite of its apparent simplicity, is slow and tends to exaggerate any hesitation or uncertainty of the subject. In comparison, the Limiting Method is much more rapid and adaptable; it is more natural and avoids unnecessary hesitation or confusion.

Subjects 1 and 2 were instructed, in accordance with the Method of Production, to move the variable until it seemed at the same distance as the standard. They often made a surprisingly wide adjustment, were then dissatisfied and altered it to a narrow adjustment. They said the wide adjustment represented apparent equality of distance, but gave apparently unequal sizes. The narrow adjustment produced the opposite effect.

A differential veiling glare is often present in fog vision, when one object is seen through more glare than another. If changing attitudes, one seeking apparent equality of size and the other equality of distance, were to alternate unconsciously in fog vision in everyday life, they might give rise to serious accidents, because the adjustments made under the two attitudes may not be the same. The remaining seven subjects were therefore tested twice, first for equality of apparent distance and then for size.

For five of the nine subjects the differential veiling glare had a measurable effect by either the size or the distance method. The other sets of readings were so erratic that their means did not differ significantly from zero, although each reading, taken separately, of course, represented an actual adjustment which might have been made in real fog vision. The irregularity of these readings is therefore important, because it indicates that the general effect of the fog was to make the subjects unreliable either in size or in distance perception or in both.

Table 1*

Subject	Method	Mean reading cm.	Number	S.E. cm.	p
3 m.	Distance	−4·1	80	1·20	<0·01
6 f.	Distance	+7·5	40	1·86	<0·01
9 f.	Distance	+4·1	40	1·04	<0·01
5 m.	Size	+2·4	24	1·05	0·05–0·02
8 f.	Size	−9·2	24	2·16	<0·01

* For S.E.'s of means and of differences of means of small samples throughout this paper, 'Student's' method was used.

Table 1 shows the mean positions chosen for the disk under more glare, measured from the other disk, for the subjects whose mean readings were statistically significant. The table shows that Subject 3 tended to place the veiled disk nearer than the other (working by distance), while Subjects 6 and 9 placed it farther (working by distance). Subject 5 placed it farther (by size) and Subject 8 placed it nearer (also by size). These results are interesting in relation to the effects of aerial perspective, which is generally supposed to make an object look distant. This point will be considered again later in the paper.

Examination of the results by the analysis of variance, for which I am indebted to Mr B. Babington Smith, who also gave much help in discussing the set-up of the experiment, enabled us to draw the following conclusions: (*a*) The eight conditions, taken together, produced significant differences for all subjects but 4 and 7, working by distance. (*b*) The time order, indicating practice, produced no appreciable effect. (*c*) Change from variable near to variable far produced significant effects for Subjects 3, 5 and 9, and doubtful for 7 and 8, working by distance; but only for 3 and 6 working by size. (*d*) Change from variable under more to variable under less glare produced significant effects for Subjects 3, 6 and 9, working by distance; and for Subject 8 by size. (*e*) Change from variable left to variable right produced significant effects for all but Subject 7 (doubtful) working by distance; and for Subjects 3, 6, 7 and 9 by size. (*f*) Smallness of variances due to interactions of conditions indicated that the factors mentioned above were effectively independent. In addition, all subjects consistently overshot or undershot the mark when working by distance; by size all undershot it but Subject 9, who was inconsistent.

From the results given above it is found that 15/21 possible changes of condition produced significant or likely effects when subjects were working by distance, but only 7/21 such effects when they worked by size. For the 2 by 2 table comparing these results with the chance frequencies, which would be equal either way, χ^2 is 6·19, giving a probability between 0·02 and 0·01. From this it may be concluded that setting for equality of distance is a more difficult task than setting for equality of size. Thus, where objects are going to be at different distances in any case, glare may be serious in upsetting judgements of distance, for example, as in driving a motor-car in a fog.

In addition to these interpretations, Mr Babington Smith also points out that, if the results are summarized in another way, it is found that subjects were sensitive to change from variable under more to variable under less glare 5/16 times; to change from variable near to variable far 7/16 times; and to change from variable right to variable left 13/16 times. From this it may be concluded that change from variable left to right is the most important single factor. It would appear to suggest a slight but appreciable defect in the apparatus. The writer, however, who has examined the apparatus carefully, thinks that the side to side effect might be due to the subjects' knowledge that the sources of light are in the right-hand compartment of the box, which might give an impression that the glare was greater on that side when it decreased from right to left than it was on the left side when it decreased from left to right. The effect might also be due to right-handedness, though in the technique for the Production Method each disk is adjusted with both hands together. The side-to-side effect persists even when the Method of Limits has been used, in later experiments not mentioned here, and it might possibly be due to eyed-ness. It is a form of the familiar 'space-error' found very persistently in psycho-physical experiments.

The factor of next importance shown in the figures above is that due to approaching the standard from an initial position nearer or farther than its distance. This is a classical factor in psycho-physical experiments, and might possibly be used as a control: effects less noticeable could possibly be ignored, while those more noticeable could be considered seriously. In terms of this control, for instance, the effect of change from variable under more to variable under less glare just passes, but only just. The general susceptibility of the subjects to changes of condition is another important aspect of the experimental

situation: Subject 3 shows most susceptibility and Subjects 1, 2 and 4 very little. This sensitiveness is an important temperamental factor, and it might be that the most practical psychological question in relation to fog vision was simply to what extent individuals were made susceptible by veiling glare to the influence of accidental and irrelevant factors in the behaviour situation.

IV. 'Normal' and 'abnormal' conditions

The veiling glare caused by illuminated fog is usually more intense on the more distant objects, but occasionally wafts of fog may reverse this condition, and these may be particularly disturbing moments, because they are exceptional or abnormal. In Exp. 1 the effects of the veiling glare were initially more intense alternately over the near and the far objects. In Exp. 2, which will now be described, these two conditions were completely separated.

Table 2

Subject	Disk with greater initial glare	Method	Mean reading cm.	S.E. cm.	p
2 f.	Near	Distance	− 3·4	1·4	0·05–0·02
3 m.	Far	Distance	+ 5·7	0·3	<0·01
	Far	Size	− 6·6	2·9	0·05–0·02
	Near	Size	+ 9·3	2·8	<0·01
4 m.	Far	Distance	+ 8·5	1·4	<0·01
	Far	Size	− 0·5	0·2	0·02
	Near	Distance	+ 6·3	0·6	<0·01
9 f.	Far	Size	+ 5·1	0·7	<0·01
	Near	Distance	− 3·4	1·1	<0·01
	Near	Size	+ 6·6	0·9	<0·01
10 m.	Far	Distance	+ 6·1	2·1	<0·01
	Far	Size	+13·9	2·6	<0·01
	Near	Distance	− 4·2	1·8	0·05–0·02
11 m.	Far	Distance	+ 7·5	1·2	<0·01
	Near	Size	+ 8·6	1·8	<0·01
12 f.	Far	Distance	+ 6·9	1·6	<0·01
	Near	Distance	+ 3·5	1·7	>0·05
14 m.	Far	Size	+ 9·4	3·4	>0·01
	Near	Size	+23·2	3·0	<0·01
16 m.	Far	Size	− 8·0	2·1	<0·01
	Near	Distance	+14·7	1·2	<0·01
	Near	Size	+ 4·4	2·1	0·05

The same apparatus was used again in the same way, but at first the greater glare was always initially over the far object, while later it was initially over the near object. Eleven subjects took part in Exp. 2. Each made twenty adjustments, five for each of four combinations of the possible conditions, omitting the four combinations in which greater glare would have been initially over the near object. This was done first by the distance and then by the size method. After an interval of several weeks similar sets of adjustments were made by nine of the subjects, using the combinations of conditions at first omitted. The other two subjects were no longer accessible. The quantities used in the calculations were the positions of the object seen under greater glare measured from the other object after the subject had adjusted the variable for equality of apparent size or distance. Details of the significant and likely mean readings are given in Table 2.

Table 3 shows the numbers of subjects giving insignificant, likely or significant mean readings with the initial glare over (1) the far and (2) the near object, for (*a*) the distance and (*b*) the size methods respectively. If these results are grouped to include more than

five readings in each cell of the table and at the same time to bring the two important parts of the experiment into comparison, Table 4 is obtained. If we calculate expected frequencies on the assumption that as many subjects would obtain insignificant as significant and likely mean readings in each part of the double experiment, χ^2 is 8/9, and there is no evidence that the subjects were more consistent under the normal than under the abnormal condition.

Table 3

p of mean reading	(1) Initial glare greater on far object		(2) Initial glare greater on near object	
	(*a*) Distance	(*b*) Size	(*a*) Distance	(*b*) Size
Greater than 0·05	6	5	3	4
0·05–0·02	0	2	3	1
0·01 or less	5	4	3	4

Table 4

	Initial glare greater on far object	Initial glare greater on near object
Significant or likely	11	11
Insignificant	11	7

In order to confirm this conclusion in another way, it is interesting to calculate the mean readings for Exp. 1, in such a way as to separate the normal and abnormal conditions. This gives a distribution of mean readings which is shown in Table 5, on the same lines as Table 4. If expected frequencies are calculated on the same assumption as for Table 4, then χ^2 for Table 5 is 10/16, and again there is no evidence that the normal condition favours consistency of adjustment.

Table 5

	Initial glare greater on far object	Initial glare greater on near object
Significant or likely	6	7
Insignificant	10	9

The results shown in Tables 4 and 5 are grouped in Table 6, to compare the frequencies of significant and likely with those of insignificant mean readings in Exps. 1 and 2, the results of Exp. 1, of course, being calculated for the normal and abnormal conditions separately. If the expected frequencies are calculated on the assumption that as many significant and likely as insignificant mean readings would have been obtained in each experiment, χ^2 is 15·4/8, and Table 6 provides no evidence that the rapid alternation of normal and abnormal conditions, as in Exp. 1, is more disturbing to vision than the change from normal to abnormal conditions after an interval of several weeks, as in Exp. 2.

Table 6

	Exp. 1	Exp. 2
Significant or likely	13	22
Insignificant	19	18

Some individual differences are interesting. Five subjects in Exp. 2 placed the less veiled disk significantly nearer and three farther by size than by distance, when the glare was initially greater on the far object; while five placed it farther and two nearer by size than by distance, when the glare was initially greater on the near object; there

were 3/18 insignificant differences between mean readings. The less veiled disk was placed significantly farther by four subjects working by distance when the initial glare was greater on the near object; three placed it nearer and one farther when working by size: here there were 10/18 insignificant differences between mean readings. In spite of these marked individual differences, a rank order correlation of +0·7 was obtained between the two rankings of the nine subjects for magnitude of difference between the settings chosen by distance and by size methods, first when the far disk and secondly when the near disk was initially the more veiled. This indicates a relatively high consistency of individual responses to the change from normal to abnormal condition.

V. Other points

From Tables 1 and 2 combined it will be seen that on 19 occasions a positive mean reading was given and on 8 a negative mean reading. This implies that 19 times the veiled disk was seen as if farther away or smaller than the less veiled disk, while 8 times it was seen as if nearer or larger and 29 times as if not consistently different. This suggests that the effect of the veiling glare might be to make the more veiled object look smaller or more distant in general rather than larger or nearer, which would accord with the usual theory of the effect of haze in aerial perspective. In order to test this hypothesis, as far as these results will admit, it may be assumed that, by chance alone, the more veiled disk will equally readily appear farther (or smaller) than, nearer (or larger) than the less veiled disk, or the same as it in size (or distance). The standard deviation is given by the formula $\sigma=\sqrt{(npq)}$, where n is 56, and, for this threefold possibility, p is $\frac{1}{3}$ and q is $\frac{2}{3}$. This standard deviation is 3·67, and the observed number, 19, of positive mean readings does not differ sufficiently from $\frac{56}{3}$ to be significant. The frequency of negative mean readings, 8, however, is less than the mean chance frequency by 2·92 times its standard deviation, and therefore has a probability of $<0{\cdot}01$. Similarly, the frequency of mean readings not significantly different from zero, 29, is greater than the mean chance frequency by 2·82 times its standard deviation, and has a probability of $<0{\cdot}01$. Thus the two experiments combined support the view that the haze is frequently inclined to have an effect so irregular that it cannot be measured by a mean reading, and is not generally inclined to make objects look large or near, though no conclusion can be drawn about any tendency it may have to make them look small or distant.

The problem of the relation of sex to accuracy or consistency may be raised in connexion with fog vision and is decidedly interesting. If all the significant and likely mean readings and all the insignificant ones are grouped separately for men and women subjects, Table 7 is obtained. The expected frequencies may be calculated on the assumption that, if there is no sex difference in consistency of adjustment, men and women will both give as many significant and likely as insignificant mean readings. Then, for this table, χ^2 is 0·85, and a confident inference may be drawn that, for these two experiments, there was no sex difference.

Table 7

	Men	Women
Significant or likely	18	9
Insignificant	16	13

Introspections and comments were obtained throughout both experiments. The most interesting of these may be noted. Subjects 3 and 6 expressed a strong preference for

working by distance; 9 and 15 by size. The others offered no comment on this point except Subject 8, who expressly stated that there was no difference in difficulty.

Subjects 3 and 6*, when the adjustment for equality of apparent distance had been made in Exp. 1, and Subjects 14 and 15, in Exp. 2 with the initial glare greater over the far object, said the more hazy disk looked smaller than the other, but in Exp. 1 Subject 4 said it looked larger. When the adjustment for apparent equality of size had been made, in Exp. 1, Subjects 3, 4 and 5* said the more hazy disk looked farther away than the other, while Subjects 6 and 7 in Exp. 1, and Subject 12 in Exp. 2, with the greater glare initially over the far object, said it looked nearer. The reports of Subjects 5 and 6, marked with an asterisk, confirm statistically measurable readings. For 11 observations $\sigma=\sqrt{(11\times\frac{1}{2}\times\frac{1}{2})}=1{\cdot}66$, and the mean chance expected frequency for two alternative possibilities is 5·5. The observed deviations, of $\pm 3{\cdot}5$, are 2·1 times the standard deviation, and have a probability of $<0{\cdot}03$. From this it may be inferred that, after adjustments for equality of size or distance had been made, if the disks then appeared different in distance or size respectively, their appearance was little or no guide to their true position.

Table 8

	(*a*) Initial glare greater on far object		(*b*) Initial glare greater on near object	
	Distance	Size	Distance	Size
Positive	5	3	3	5
Not significant	4	3	3	4
Negative	0	3	3	0

Mr Babington Smith has pointed out that if the sign of the mean reading under the several conditions is noted, Table 8 may be constructed. This table suggests an interesting possibility. (*a*) When the initially greater glare is on the far object, a positive mean reading indicates that the subject stopped short, since the measurement is made from the object under less glare. It is to be noted that none of the subjects moved the variable too far when working by distance. (*b*) With the initial glare greater on the near object, however, it is a negative mean reading which indicates that the subject has stopped short, and it is found that none of the subjects moved the variable too little in his approach to the standard when setting by size. Thus there is a possibility of a tendency to make too little movement towards the standard disk when setting by distance, and in contrast to make too much movement when setting by size. While this cannot be treated as more than a possibility, it suggests that the method of making settings has a considerable effect on the results.

VI. Uniform glare and no glare

At the time that the work described was planned and carried out it was thought that the experiments contained their own controls in themselves because a disk seen under much glare was compared with another disk seen under little glare. It was clear from other work(1, 2) that phenomenal regression for size is for most subjects progressively reduced by increases in veiling glare when objects are seen at different distances, but it was assumed that no differential effects would be produced by uniform binocular glare over objects of the same size and seen at the same distance. The results of Exps. 1 and 2 were so unexpected that two control experiments were carried out with three of the

original subjects who were still available. These subjects are experienced and have been doing similar work during the interval since the earlier experiments were finished, and reliance can be put on their work.

The first control experiment was carried out with no glare, but with other conditions and method exactly as in the first experiment. The results are shown in Table 9. Subject 2 made no significant readings at all by distance. None of her mean readings, measures of the average setting of the variable taken from the standard, neither the grand mean, or the mean for the variable left or right, or for the variable near or far, differed significantly from zero. The actual settings were extremely small. Thus the grand mean was +0·175 cm. with a standard error of 0·299 cm. The other two subjects, 3 and 4, gave very small but consistent grand mean readings, and were affected by a small space error, since both tended to place the variable a trifle nearer than the standard when it was to the left.

Table 9. *No glare. By distance*

Subject	Condition	Mean reading in cm.	Number	S.E. in cm.	p
3 m.	General	−1·33	20	0·29	<0·01
	Variable left	−2·05	10	0·11	<0·01
4 m.	General	−0·95	20	0·40	0·05–0·02
	Variable left	−2·50	10	0·17	<0·01

Under the condition of no glare none of the three subjects was able to make the adjustment for size. The variable did not change in apparent size at all for Subjects 2 and 4, however much the string was pulled in either direction. It changed in apparent distance, but remained the same size as the standard within the limits fixed by the apparatus, namely ± about one-sixth of the distance of the standard. According to the inverse square law apparent size would vary as the inverse of the square of the distance. Thouless(6) has shown that this does not hold in ordinary perception, and has substituted the Law of Phenomenal Regression, according to which the apparent size of an object is a compromise between its real size and the size it should have according to the strict laws of perspective. In the present experiment apparent size remained unchanged over a wide range of apparent distances.

For Subject 3 the effect was even more unexpected. The variable always looked larger, even when known to be the same size and when seen to be at the same distance. Such a result might be distrusted unless it was obtained with an experienced subject.

In the uniform glare experiment two 40 W. lamps were used, one in each lamp socket of the apparatus, and, when switched on together, they produced a uniform haze of about the same intensity as the mean intensity of the differential glare produced by one 60 W. lamp at the side. The results of this experiment are given in Table 10. They contrast sharply with those shown for the experiment with no glare in Table 9. Subject 2 gives no measurable readings by distance, but by size she gives a measurable grand mean, she is affected by the space error and consistently overshoots the mark when the variable is initially too near. Subject 3 gives a measurable grand mean by distance, is affected by the space error and overshoots the mark when the variable is initially too far. Subject 4 gives no measurable grand mean, but is affected by space errors, right and left, both by size and by distance.

For Subject 3, in the uniform glare experiment, the size method was again impossible,

because the variable disk always looked larger even when known to be the same size and when seen at the same distance.

Table 10

Subject	Condition	Mean reading cm.	Number	S.E. cm.	p
2 f.	Size: general	+2·67	20	0·97	0·02–0·01
	Size: variable left	+3·85	10	1·54	0·05–0·02
	Size: variable near	+3·10	10	1·25	0·05–0·02
3 m.	Distance: general	−2·80	20	0·65	<0·01
	Distance: variable left	−3·70	10	1·35	0·05–0·02
	Distance: variable right	−4·10	10	0·70	<0·01
4 m.	Distance: variable right	+1·05	10	0·39	0·02
	Distance: variable left	−3·00	10	0·29	<0·01
	Size: variable right	−2·70	10	0·16	<0·01
	Size: variable left	−1·85	10	0·30	<0·01

In Table 11 the results of these two control experiments are summarized. For this table we may assume that pure chance would have given equal numbers of significant or likely, of insignificant and of impossible measurements, and, on this hypothesis, χ^2 is 11·2, n is 2 and p is less than 0·01. The conclusion may be drawn with confidence that, for these three subjects, the effect of a uniform glare as compared with no glare is to make the subject much more sensitive to a number of 'irrelevant' conditions, such as the initial position of the variable and the space error. This confirms the impression given by the first two experiments.

Table 11

	No glare	Uniform glare
Significant or likely	4	10
Not significant	11	15
Impossible	15	5

One of the most remarkable effects of the veiling glare is to separate the integration of sizes and distances in perception. Thus the size and distance methods become possible as distinct modes of adjustment when there is a haze. They cannot be separated in normal perception because size tends either to be unaffected, or to follow the law of phenomenal regression, or to be affected by psychological conditions such as the fact that the subject is in control of the object in question; while distance undergoes more definite change. This accords with the biology of adaptive behaviour. It is more important to be able to estimate accurately the relative distances of objects we may be trying to grasp than their relative size.

The results of these control experiments confirm a conclusion drawn from Exp. 1, namely, that Subjects 2 and 4 show little susceptibility to the changes of condition in the experimental situation, while Subject 3 is very susceptible. The results and conclusions of all these experiments will require confirmation by further experiments and tests.

VII. SUMMARY AND CONCLUSIONS

An apparatus was set up to study the effects of a differential veiling glare in binocular vision. Two critical experiments were performed, and also two control experiments.

In the first experiment two subjects were found to be inclined to make different adjustments of apparent equality of two disks, one of which was seen through more haze than the

other, one adjustment when they worked by size and the other when they worked by distance. The seven other subjects were asked to separate the size and distance methods. Only five out of a possible sixteen mean readings were statistically significant, no subject gave significant mean readings by both methods, and four subjects out of nine gave no significant mean readings at all. This indicates the disturbing nature of the haze. The change from variable left to variable right was the most important single factor affecting the subjects taken as a group. The change from variable near to variable far was next in importance, and the change from variable under more to variable under less glare was the third. An important point was a temperamental factor: the degree of susceptibility of different subjects to the changes of experimental condition.

In the second experiment the subjects were asked to make the adjustments for apparent size and apparent distance under the 'natural' condition when the nearer disk is always initially under less glare than the other, and then, after several weeks, to repeat the experiment under the 'unnatural' condition when the nearer disk is initially under greater glare. Nine subjects did this, and two more did the first part only. There was no tendency to make the adjustment more consistently under the normal than under the abnormal condition, nor any tendency for the adjustments to be more reliable when the normal and abnormal conditions were separated by several weeks than when they alternated rapidly.

Marked individual differences were found in both experiments, though the subjects of the second experiment showed a high consistency of individual response to the change from normal to abnormal condition.

Grouped results of the two experiments show no sex difference in reliability of mean readings. They support the theory that haze is not generally inclined to make objects look larger or nearer, but show that it is readily inclined to have a very irregular effect. Comparison of subjects' introspective estimates with measurable results suggests that the haze makes appearance a very uncertain guide to actual size or distance of objects. There is a possibility that individuals tend to stop short in their adjustments when setting by distance and to go too far when setting by size.

Two control experiments were carried out with three of the subjects. These experiments were (*a*) without glare and (*b*) with uniform glare. They confirm the conclusion made probable by the other experiments, that veiling glare tends to make a great increase in the sensitivity of the subjects to 'accidental' conditions of response, and this may be a more important effect of the haze in size and distance perception than 'aerial perspective' itself considered as a distance cue. The presence of uniform veiling glare has markedly individual effects on the different subjects concerned. It also tends to dislocate the normal integration of size-distance relationships and appearances in perception, so that apparent size and distance become separate factors in fog vision, while in normal vision they are united in the effects of the law of phenomenal regression for size.

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INFLUENCE OF SHORT REST PAUSES ON FLUCTUATIONS OF ATTENTION

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I. *Introduction* (p. 162). II. *The experiment* (p. 164). III. *Full series* (pp. 164–170): (*a*) *Full curves* (pp. 164–166); (*b*) *The grand total full curve* (pp. 166–167); (*c*) *Shapes* (pp. 167–170). IV. *Extracted curves* (pp. 170–171). V. *Summary* (p. 171). *References* (p. 171).

I. Introduction

The research described in the present paper(3) assumes Philpott's(1) theory of the work curve. The curve of output for a given task (when plotted in detail) can, according to this theory, be expressed as the sum of a series of cycles running throughout the length of the curve. Successive pulsations of these cycles are not of constant length, but increase with time in geometric progression.

For example, in one of the work curves of the present research (Fig. 1*A*) troughs marked by a cross occur at about 26, 63½, 154, 372 and 900 sec. from zero. These figures are in geometric progression, each being 2·42 times the one next preceding. We may say that the curve contains a cycle of geometric ratio 2·42. By plotting the curve on logarithmic graph paper geometric pulses are made to appear of constant length throughout, successive troughs of the ratio 2·42 cycle being equidistant. Other geometric cycles are present in the curve besides that of ratio 2·42. Work curves contain a number of such cycles, each with its own characteristic ratio or wave-length.

A work curve usually has a dominant cycle, which may be revealed by smoothing the curve. The argument for smoothing is as follows: Fig. 2*A* is a single periodic cycle. Portions of this curve lying between consecutive thick vertical lines marked *A* (i.e. intervals marked *ABA*) are each a whole duration or crest-trough unit of the cycle. If we calculate average output per whole duration, the curve will be completely smooth, since output per crest-trough unit is constant. The whole duration smoothing of Fig. 2*A* is shown in Fig. 2*B*. On the other hand, part duration smoothings in which we calculate output every, say, half duration or half crest-trough unit should throw the cycle into prominence. Portions of curve 2*A* lying between consecutive vertical lines whether thick or dotted (i.e. intervals marked *AB* or *BA*) are each a half duration of a single crest-trough unit. Fig. 2*C* shows the half duration smoothing of Fig. 2*A*. It loops by twos, the cycle being thrown into prominence. Similarly, one-third duration smoothings will loop by threes, quarter duration smoothings will loop by fours, and so on.

Here is a method of calculation, which if applied to work curves will make periodic cycles manifest. If a work curve contains a dominant cycle, whole-duration smoothings in terms of that cycle should tend to be completely smooth, whilst part-duration smoothings should loop rhythmically. Whole and part duration obtained in practice will not be so monotonously smooth nor undulate so gracefully as those shown in Fig. 2, since work curves are complex.

Philpott(1) smoothed most of his work curves in this way. His results suggested that the curve of output, when plotted in detail, is characterized by cycles of geometric wave-length.

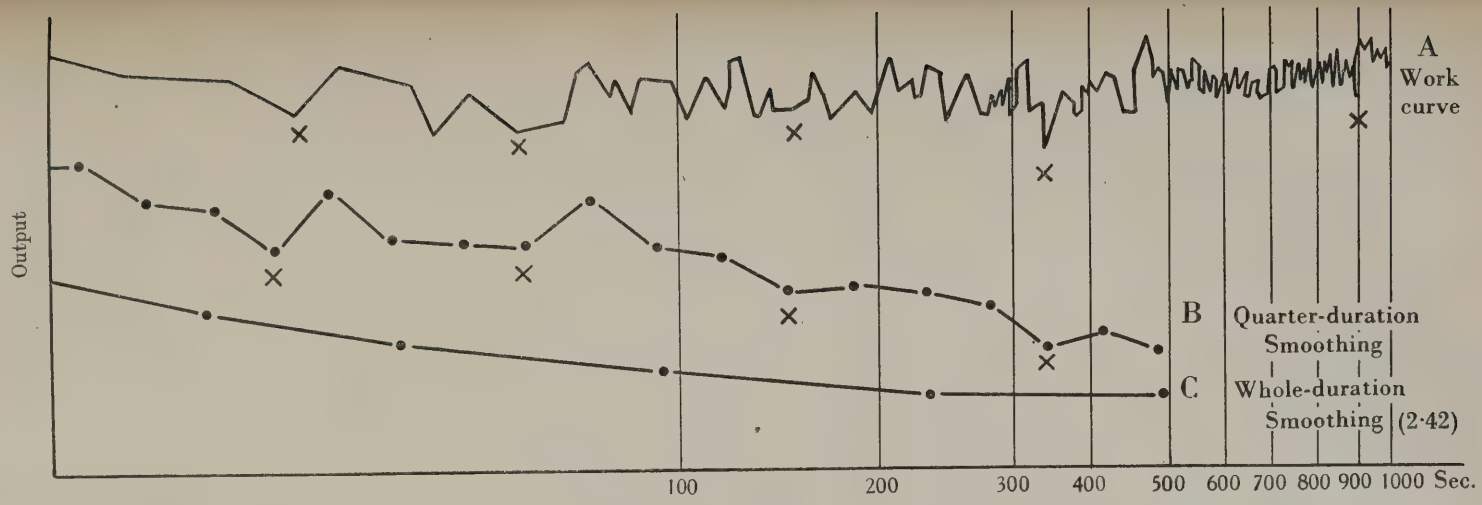

Fig. 1.

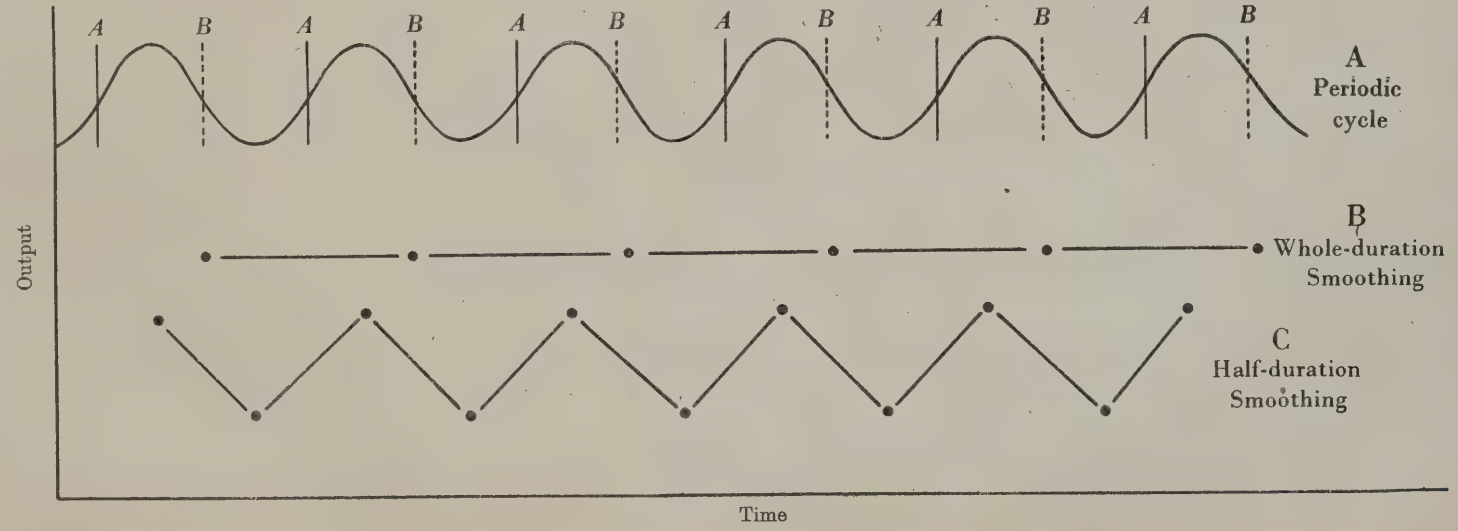

Fig. 2. Work curve smoothings.

II. The experiment

The present research was designed to examine the effect of interruption by rest upon such geometric cycles.

The subjects were 100 children, aged 11–14, drawn from London Elementary Schools. The task was simple addition, performed upon Kraepelin sheets, containing more than 200 rows of 13 digits each. Instructions were to add 2 to each digit in turn, working horizontally along the rows, and to write the answers at the side of the appropriate digits. The subjects worked continuously, except, of course, during rest pauses, commencing a fresh row of work every 5 sec. This enabled work curves to be plotted in considerable detail, time intervals being of only 5 sec. duration. Each child had four sittings at the task and at each sitting his work was broken by a single rest pause, of either 5, 10, 20 or 40 sec. duration. There were in all 400 individual curves, that is, curves based upon a single attempt at a task.

III. Full series

(*a*) *Full curves.* The first question the research had to answer was whether geometric cycles cut across periods of rest. Curves showing output for the full task, including rest, were called full curves. They presumably contain any cycles which cut across the rest pauses.

The full curves of the research, obtained by summing 32–34 individual curves, thus showing the combined output of 32–34 individuals, were smoothed by the method described above in order to throw into prominence any cycles running through the full task.

Fig. 1 *A* is one of the full curves of the research, showing output for the full task with a rest of 10 sec. duration.[1] The dominant cycle of the curve is of geometric ratio 2·42. Troughs of this cycle, with the exception of the last two at 372 and 900 sec., are not prominent and would not be considered important on a cursory examination of the curve. The dominant cycle is not always obvious to the eye. It is necessary to smooth a curve before one can be at all certain. Smoothings (Fig. 1 *B*, *C*) show that it is the ratio 2·42 cycle which is dominant.

The whole-duration smoothing (Fig. 1 *C*) is so smooth that it is almost a straight line. The quarter-duration smoothing (Fig. 1 *B*), which should loop by fours, does so with a well-marked rhythm. A geometric cycle of ratio 2·42 appears to be present and to run through the full curve, thus cutting across the periods of rest the curve contains.

Difficulties arose when smoothing full curves containing a rest of either 20 or 40 sec. As would be expected, the subjects usually gave increased output after rest. Since the research was concerned with possible cycles running through the curves, it had been arranged that the subjects should not rest simultaneously but at twenty consecutive points. This ensured that increments after rest should be evenly distributed, and not

[1] Certain features of the full curves should be noted. It has been arranged (for reasons dealt with later) that subjects should not rest simultaneously but in twenty consecutive groups, that is, at twenty consecutive points in the curve. This means that although every subject had one period of rest there was no point at which all subjects were resting together. No gaps, therefore, indicating rest pauses, appear in the full curves. Moreover, differences in total output at those points where some subjects were resting do not show in the curves, which are of average, not total, output. Lastly, output for the first 10 sec. is not shown in the curves. This is partly because it is inconvenient to plot the first 10 sec. work on logarithmic graph paper and partly because pulses of geometric cycles, being of increasing wave-length, are extremely small near the commencement of work, where they have not diverged sufficiently to become distinguishable from one another. It was not necessary therefore to plot the first 10 sec. work. Output is at first relatively high. For example, during the missing 10 sec. Fig. 1 *A* is descending rapidly. The portion of this curve shown in the diagram is, as regards its general slope, the horizontal end of a rectangular hyperbola.

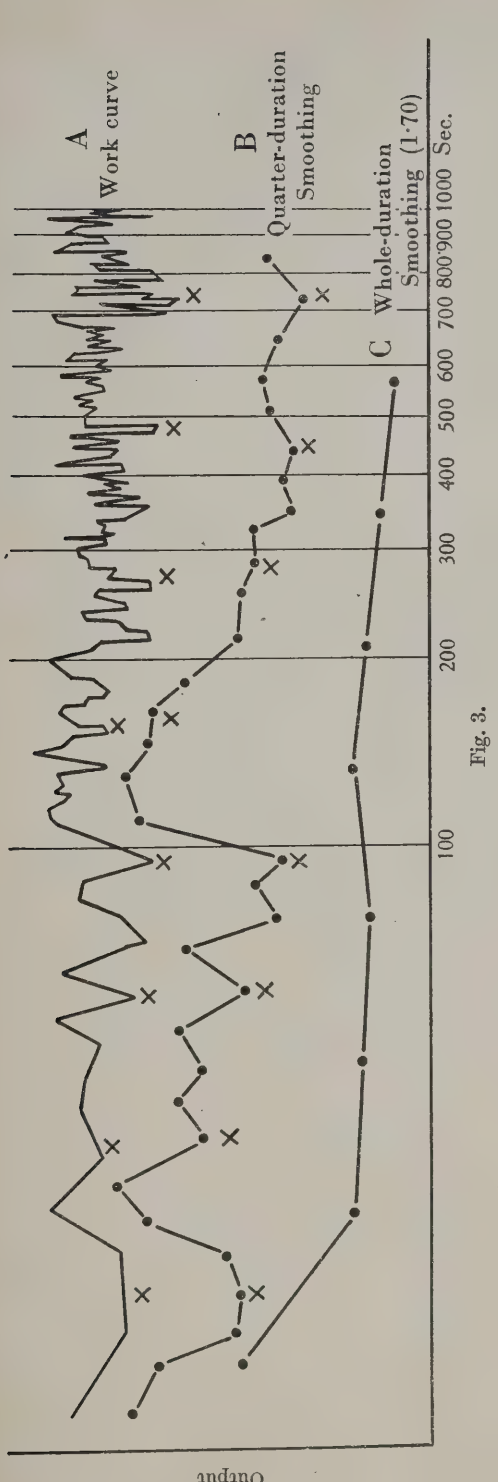
A
Work curve
B
Quarter-duration
Smoothing
C
Whole-duration
Smoothing (1·70)
100
200
300
400
500
600
700
800
900
1000
Sec.
Output

Fig. 3.

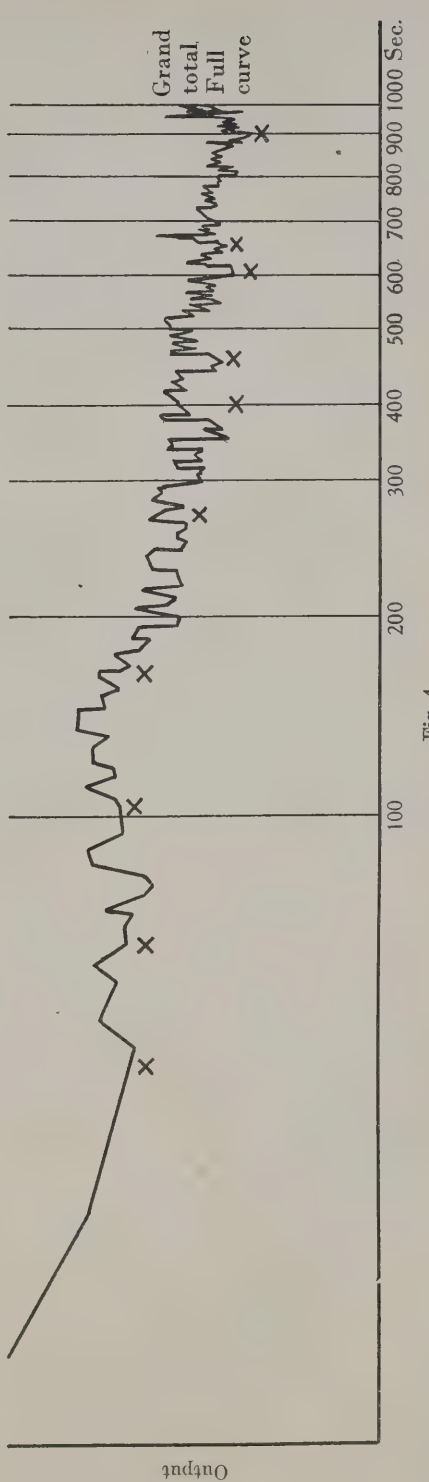
Grand
total
Full
curve
100
200
300
400
500
600
700
800
900
1000
Sec.
Output

Fig. 4.

fall at any one point in the curve. After 40 sec. rest, for example, some subjects recommenced work at 100 sec. from the beginning of the task, some at 105 sec., some at 110 sec., and so on up to the 195th sec. On summation, their various initial spurts after rest nevertheless appear as a broad bulge in the curves. For example, in Fig. 3*A*, a full curve showing output for the full task with a rest of 40 sec., successive troughs of the dominant cycle of ratio 1·70 have been marked by a cross. The bulge due to increased output after rest, which shows clearly in the quarter-duration smoothing (Fig. 3*B*), extends from about the 100th to about the 400th sec. The 40 sec. rest pauses were the longest of the research, and it is after rest pauses of this length that increased output may be expected to be most marked.

The bulge has interfered with the work curve smoothings, showing in the whole-duration smoothing (Fig. 3*C*), which is consequently not completely smooth. Quarter-duration loopings (Fig. 3*B*) are replaced by a long hump within what may be called the 'zone of bulging'.

Can it be said that a cycle of ratio 1·70 is present in the curve, despite the fact that the smoothings have largely broken down? The evidence suggests that such a cycle is present since, first, successive troughs of the ratio 1·70 cycle are prominent in the work curve (Fig. 3*A*). They are not influenced by the bulge, except that the trough at the 151st sec. is raised in the absolute scale, being lifted above troughs of the cycle lying in other portions of the curve. Secondly, the quarter-duration smoothing (Fig. 3*B*) loops regularly by fours during the first 100 sec. Within the 'zone of bulging' loopings are replaced by a long hump, although very distorted fourfold loopings may be present. By 272 sec. loopings are beginning to reappear. After the 400th sec., that is, after the bulge, the curve has settled down and is looping normally. Thirdly, the whole-duration smoothing (Fig. 3*C*) is smooth, except in the zone of bulging.

The ratio 1·70 cycle is thus present before and after the zone of bulging. A cycle of geometric wave-length appears to run through the curve, cutting across the rest pauses. The cycle is distorted within the zone of bulging, that is, by increased output after rest.

The two work curves already described are typical of the full curves of the research. After 5 or 10 sec. rest bulging was largely absent and smoothings were easily obtained. After 20 or 40 sec. rest, bulges were of considerable amplitude, geometric cycles being distorted within the zone of bulging. There were twelve full curves in all. Smoothings in every case suggested the presence of geometric cycles cutting across periods of rest.

(*b*) *The grand total full curve.* The next step was to examine the grand total full curve of the research, obtained by summing the twelve full curves.

This curve contained 400 individual curves. A curve based upon some hundreds of records cannot be smoothed satisfactorily, since, being the sum of many curves, it contains most of the possible geometric cycles, which interfere with one another. Instead of any one cycle being dominant throughout the length of grand total curves, one cycle may be dominant in one part of the curve, and another cycle dominant in another part. Obviously it is impossible to smooth such curves in terms of a single cycle. On the other hand, advantage may be taken of the fact that, as more and more records are summed, all the possible cycles tend to be present in a grand total curve in due proportion, since the greater the number of records in a curve, the greater the chance that it will contain a representative sample of cycles of all lengths.

In an attempt to construct such a standard curve, Philpott(2) summed all the possible

cycles, thus finding the most probable trough points in a curve of heavy weight. These most probable trough points belong to the various cycles which become dominant in different parts of the curve when all the possible cycles are summed. This provides us with a criterion of geometric periodicity in curves containing too many records to be smoothed satisfactorily. If the deepest troughs in a grand total curve tend to fall at the most probable trough points, we may infer that it is based upon cycles of geometric wave-length.

The grand total full curve of the research (Fig. 4*A*) was therefore examined for deep troughs at the most probable trough points. The ten most probable troughs have been marked by a cross. Except for the first two troughs at 38 and 63 sec., they are not the most prominent troughs up to 400 sec., so that the criterion is not satisfied in this portion of the curve. On the other hand, after the 400th sec., the four most probable points are the four deepest troughs.

Why then should the criterion be exactly satisfied after the 400th sec., and yet break down almost completely before that point? It can best be explained, as in the case of the 40 sec. rest curve (Fig. 3), in terms of distortion due to bulging after rest. Bulges in the full curves give, on summation, a bulge in the grand total full curve extending from the 80th to about the 400th sec. Troughs lying within this raised portion or bulge are raised relative to troughs in other portions of the curve. Troughs which would have been prominent were it not for bulging will not stand out so clearly.

The criterion of deep troughs at the most probable trough points in grand total curves, being based upon the prominence of given troughs, is thus somewhat invalidated in the case of curves which contain a bulge due to increased output after rest.[1] The important point is that it is just where the grand total curve is bulging, that is, between about the 80th and the 400th sec., that the most probable trough points are not prominent. Before 80 and after 400 sec., that is, except in the zone of bulging, the criterion is satisfied.

Is it to be said that geometric cycles are not present in the grand total full curve, or that they are present and have been distorted by bulging? The evidence suggests that cycles have been distorted by the bulge in the curve, since distortion seems to be exclusively confined to the zone of bulging. Before and after this zone the criterion of deep troughs at the most probable trough points appears to be satisfied. In this respect, the grand total full curve resembles the 40 sec. rest curve, in which the criterion of rhythmical loopings broke down exclusively within the zone of bulging.

(*c*) *Shapes.* Confirmation was sought of the finding of periodicity by Philpott's method of shapes. By this method, work curves are cut into successive sections of one period length each. These sections are placed on top of one another and summed, to give the outline of an 'average period' or 'shape'. All the curves are cut at the same points in the time scale.

[1] It might be objected that if the effect of bulging is simply to raise a portion of the curve, to lift it bodily, as it were, above the remaining portion of the curve, individual troughs within that raised portion will not thereby be made less prominent, although they will be raised in the absolute scale. The present curve is, however, a grand total curve, obtained by summing 400 individual curves. Similarly, the bulge in the grand total curve may be called the 'grand total bulge', representing the sum of 400 individual bulges, which may differ among themselves both in amplitude and length. This grand total bulge, considered in isolation from the rest of the curve, may be irregular in outline, relatively raising and lowering individual troughs in fortuitous fashion. We cannot say whether certain troughs would have been more prominent or less prominent in the absence of bulging. Distortion was certainly not so marked in the 20 and 40 sec. rest curves. This may be ascribed to the fact that these curves were based upon fewer records than the grand total curve was, distortion due to the summation of individual bulges being less marked in consequence.

Fig. 5 shows some shapes or average periods from the curves of the present research. Each shape has seven points which are numbered.[1] Shapes *A*, *B* and *C* agree with one another in the position of their deepest troughs, which fall at point 3 in all three shapes, whereas shape *D* has its deepest trough at point 2.

All shapes will have a deepest trough, except in the almost inconceivable case of a shape being a horizontal straight line. On a chance basis, shapes from different curves will have their deepest troughs at different points. If, however, the various curves all tend to contain the same periods, their shapes will tend to agree in the placing of their deepest trough points.

As we saw in connexion with the grand total full curve, if the hypothesis of geometric periodicity is correct, weak curves based on a few records will vary more amongst themselves in respect of their geometric cycles than will strong curves, obtained by summing a relatively large number of records, since the stronger the curve the greater the chance that it will contain a representative sample of cycles of all lengths, strong curves thus tending to approximate to the 'standard outline'.

Strong curves should, by virtue of their common resemblance to the standard curve, tend to agree with one another, while weak curves should tend to vary. We may therefore expect to find signs of variability in shapes from weak curves and signs of agreement in shapes from strong curves.

A measure of agreement between shapes, in respect of their constituent cycles, is given by their various 'deepest-trough positions'. Fig. 6 is a frequency diagram showing the distribution on the 1–7 scale of the 'deepest-trough positions' of the twenty-four strong shapes of the research. It shows that the strong shapes agree in so far as their deepest troughs tend to fall at regions 2–3 or 6–7 on the base-line scale. Bimodal grouping is consistent with the general hypothesis and is, in fact, to be expected (see Philpott(1), pp. 82–3). None of the twenty-four strong shapes has its deepest trough falling at either points 4 or 5 on the 1–7 scale. The odds against this occurring by chance are about 60 to 1, if account be taken of the fact that 4 and 5 are consecutive values. Philpott(1), using the same method, found a similar type of distribution of 'deepest-trough positions' for his strong shapes. In contrast with the strong shapes, 'deepest-trough positions' of the weak shapes, also shown in the frequency diagram of Fig. 6, vary to such an extent that they seem to be chance determined, being distributed about all points on the 1–7 scale.

Thus it may be said that the general hypothesis is confirmed in so far as weak shapes tend to vary while strong shapes tend to agree in the positions of their deepest troughs.

Tendencies towards agreement or variability may be expressed quantitatively by calculating the standard deviations of such distributions as shown in the frequency diagrams of Fig. 6. If weak shapes vary amongst themselves, variability will show as high standard deviations, and if strong shapes tend to agree amongst themselves, agreement will show as lower standard deviations. S.D.'s of the distribution of 'deepest-trough positions' of the full shapes were:

Weak (3 records apiece)	1·92
Medium (10–12 records apiece)	1·85
Strong (32–34 records apiece)	1·46

[1] Actually, shapes were calculated, not for a single period, but for two consecutive periods, as shown by the numbers at the foot of Fig. 5.

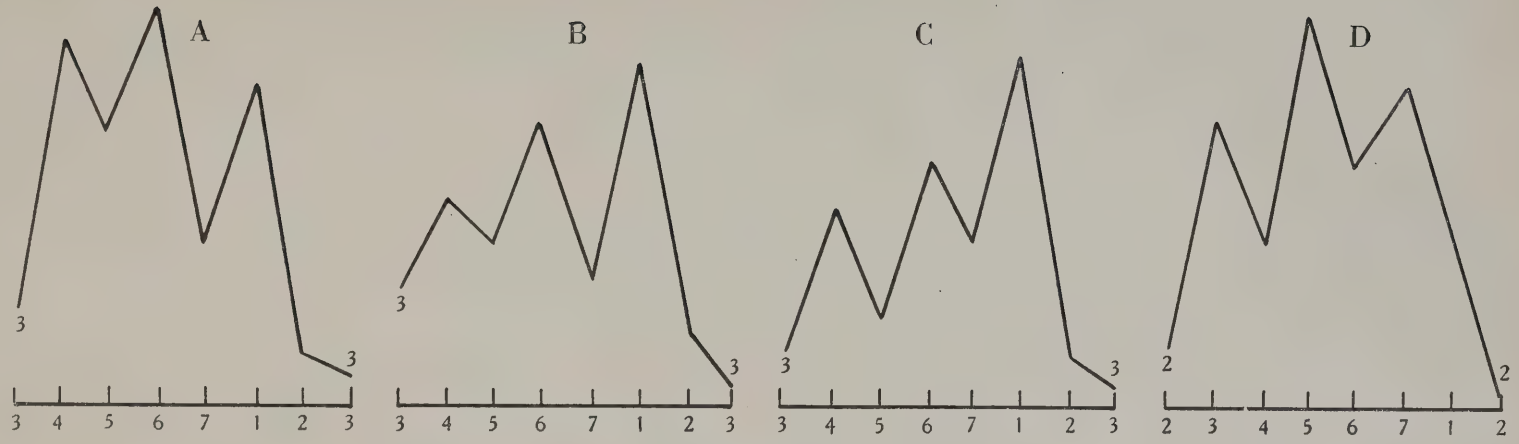

Fig. 5. Shapes.

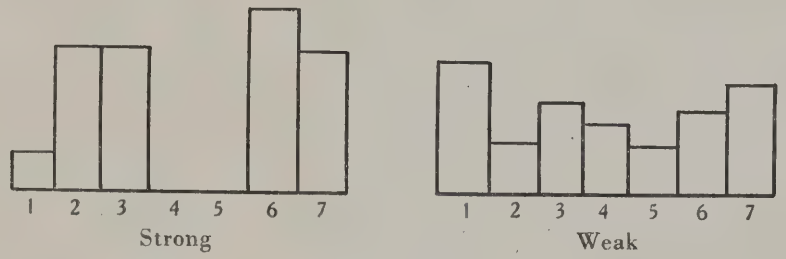

Fig. 6. Shapes.

Since these values do become smaller as more and more records are summed, it may be said that weak shapes vary more amongst themselves than do strong shapes, thus confirming, in accordance with the general argument, the finding of periodicity in the full curves.

IV. Extracted curves

The next question the research had to answer was whether fresh cycles are engendered immediately after rest, in addition to those running through the full task.

In order to answer this question work curves were summed in such a way as to show only work performed after rest, time intervals being renumbered to make the point of resumption of work after rest zero. Such curves were called extracted curves. Evidence was sought of periodicity from the extracted curves by the methods applied to the full curves; that is, first, by work curve smoothings, secondly, by examination of the grand total (extracted) curve, and thirdly, by shape analysis.

Smoothings of the extracted curves revealed the presence of geometric cycles, whole durations tending to be completely smooth and part durations looping rhythmically as in the full curves.

The grand total extracted curve should, if it contains geometric cycles, tend to have its deepest troughs at Philpott's 'most probable trough points'. The grand total full curve, as we have seen above, did not wholly conform to this criterion, owing to bulging due to increased output after rest. This difficulty of bulging after rest does not occur in the case of extracted curves, in which the point of resumption of work after rest is called zero time and increased output after rest is absorbed mainly during the first few seconds. The grand total extracted curve should have the normal work curve characteristic of initial fall and be free from bulging.

Philpott's most probable trough points in the grand total extracted curve adhered fairly closely to his analysis, deep troughs tending to be situated at these points, the criterion of deep troughs at the most probable trough points in grand total curves being satisfied.[1]

Extracted curves were also analysed by the method of shapes. As we saw in connexion with the full shapes, strong shapes, based on a relatively large number of records, should show lower standard deviations in respect of their 'deepest-trough positions' than should weak shapes, based on a few records. Standard deviations were:

Weak (3 records apiece)	1·95
Medium (10–12 records apiece)	1·85
Strong (32–34 records apiece)	1·55

As these values became smaller as the number of records increased, it may be said that weak shapes vary more among themselves than do strong shapes, thus confirming the finding of geometric periodicity in the extracted curves.

[1] The only serious discrepancy was at the 500th sec., where a very deep trough was placed. A deep trough at 500 sec. is unusual in any work curve. In a grand total curve it is most unexpected, since it belongs to no cycles which should be prominent in such curves. There is no trough at 500 sec. in the grand total full curve (Fig. 4*A*), for example. The appearance of this trough in the grand total extracted curve was possibly due to the method of administering the test. Rows of figures in the original work sheets were numbered 1, 2, 3, etc., starting at the point of resumption of work after rest. After rest, all subjects were working on line '1' together, on line '2' together, and so on. Since they were allowed 5 sec. per line, it follows that the 500th sec. in the extracted curves represents the 100th line of work after rest. Presumably, as subjects approached the 100th line they slowed down, so that a trough appeared at the 500th sec.

The evidence thus suggests that the extracted curves, showing output after rest, contain periodic cycles of geometric wave-length. One of the main findings of the research therefore was that geometric cycles begin immediately after rest.

V. Summary

The curve of output, when plotted in detail, is characterized by cycles of geometric periodicity. The present research was designed to examine the effect of interruption by rest pauses (of either 5, 10, 20 or 40 sec. duration) upon such geometric cycles. The findings were, first, from examination of the full curves, that geometric cycles run through the complete task, cutting across periods of rest; and secondly, from examination of the extracted curves, that other geometric cycles commence immediately after rest.

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THE APPLICATION OF THE MAXIMUM LIKELIHOOD METHOD TO FACTOR ANALYSIS

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I. *Estimation of factor loadings* (pp. 172–174). II. *Testing significance* (pp. 174–175). *References* (p. 175)

I. Estimation of factor loadings

In two previous articles(1, 2) the writer has put forward a new method of estimating the loadings of a set of tests in a number of factors, given the observed correlations between these tests. This method of factor estimation, though somewhat more laborious than the majority of those at present in use, possesses certain advantages: in particular, there exists, in cases where the number of individuals tested is reasonably large, a satisfactory method of deciding how many factors are required to account for the scores obtained. As the process of estimating the factor loadings was previously given in mathematical form it is intended in this paper to show how it may be applied to actual data and to explain the arithmetical calculations required.

As an example we shall consider some of the data collected for the Scottish Council for Research in Education by Miss Macmeeken during 1935–6; an analysis of which data has already been given by Prof. Godfrey H. Thomson(3). For our purposes we shall use only the results obtained from the representative sample of 443 boys, and we shall confine ourselves to a consideration of the correlations between eight of the variables. These eight variables are as follows:

(1) Binet I.Q.
(2) Manikin Test (time).
(3) Stutsman Picture Test (time).
(4) Red Riding Hood Test.
(5) Healy Picture Completion Test.
(6) Knox Cube Imitation Test.
(7) Cube Construction Test.
(8) Kohs Block Design.

The correlation coefficients (with the effect of age eliminated) between these tests are given, correct to three figures, in Table 1.

Table 1

	1	2	3	4	5	6	7	8
1	1·000	0·312	0·405	0·457	0·500	0·350	0·521	0·564
2	0·312	1·000	0·460	0·316	0·279	0·173	0·339	0·288
3	0·405	0·460	1·000	0·394	0·380	0·258	0·433	0·323
4	0·457	0·316	0·394	1·000	0·460	0·222	0·516	0·486
5	0·500	0·279	0·380	0·460	1·000	0·239	0·441	0·417
6	0·350	0·173	0·258	0·222	0·239	1·000	0·302	0·262
7	0·521	0·339	0·433	0·516	0·441	0·302	1·000	0·547
8	0·564	0·288	0·323	0·486	0·417	0·262	0·547	1·000

In the diagonal cells we have placed unities, in readiness for the subsequent calculations.

In addition to a factor corresponding to general intelligence it has been suggested that at least one other factor, possibly a speed factor, is present. As a hypothesis, therefore, we shall assume that two general or group factors are present as well as specifics, and we shall proceed to estimate the factor loadings of the eight tests in these two factors by the method of 'maximum likelihood'. Having found these we are then able to test

whether our original hypothesis was justified or whether three or more factors are required to account for the above set of correlations.

The method of estimation about to be described is one of successive approximations. Each successive step in the calculations gives a set of factor loadings which are nearer to the final values than those of the previous set. To start the process it is only necessary to guess or to find by some means (e.g. by a centroid analysis) first approximations to the factor loadings. Any set of figures within reason will serve the purpose, though of course the better the approximation the fewer steps in the calculation will be needed. For illustration we shall take as first approximations to the factor loadings the set of values given in Table 2.

Table 2

Trial loading in	Tests 1	2	3	4	5	6	7	8
Factor I	0·73	0·50	0·66	0·66	0·62	0·40	0·73	0·70
Factor II	0·17	−0·27	−0·47	0·08	0·06	0·02	0·10	0·29
Specific variance	0·4382	0·6771	0·3435	0·5580	0·6120	0·8396	0·4571	0·4259

Below the loadings are written the corresponding first approximations to the specific variances (the total variance of each test being taken to be unity). These figures represent the proportion of the variance of each test which is estimated to be due to specific factors and also to errors of observation; they are found by subtracting from unity the sums of squares of the loadings for each test.

The calculations necessary for obtaining second approximations to the loadings in factor I may now be set out as in Table 3. The first row of figures, row (*a*), is found by

Table 3

(*a*)	1·666	0·738	1·921	1·183	1·013	0·476	1·597	1·644
(*b*)	5·647	3·895	5·132	5·129	4·830	3·100	5·647	5·412
(*c*)	4·917	3·395	4·472	4·469	4·210	2·700	4·917	4·712
		$h_1^2 = 45{\cdot}724$			$1/h_1 = 0{\cdot}14789$			
(*d*)	0·727	0·502	0·661	0·661	0·623	0·399	0·727	0·697

dividing the trial loadings in factor I by the corresponding specific variances. The figures in row (*b*) are then given by the inner products[1] of row (*a*) with the successive rows (or columns) of the correlation table printed above, and row (*c*) is obtained by subtracting from the figures in row (*b*) the corresponding loadings in factor I. The quantity h_1^2 is given by the inner product of rows (*b*) and (*c*), and hence, taking the square root of the

Table 4

(*e*)	0·388	−0·399	−1·368	0·143	0·098	0·024	0·219	0·681
(*f*)	0·330	−0·560	−0·980	0·150	0·113	0·038	0·190	0·580
		$p_1 = -0{\cdot}0234$						
(*g*)	0·177	−0·278	−0·495	0·085	0·068	0·027	0·107	0·306
		$k_1^2 = 1{\cdot}1080$			$1/k_1 = 0{\cdot}9500$			
(*h*)	0·168	−0·264	−0·470	0·081	0·065	0·026	0·102	0·291

reciprocal of this quantity, we find $1/h_1$. Finally, row (*d*) is obtained by multiplying the figures in row (*c*) by $1/h_1$, or 0·14789. The resulting numbers are then second approximations to the loadings of the tests in factor I.

A similar process is followed in obtaining second approximations to the loadings in factor II, only a slight modification being needed. The necessary calculations are shown

[1] By 'inner product' of two series of numbers is meant the sum of their products in pairs. Thus the inner product of the two sets, $x\ y\ z$ and $X\ Y\ Z$ is $xX + yY + zZ$.

in Table 4. Row (*e*) is found by dividing the trial loadings in factor II by the corresponding specific variances, while the numbers in row (*f*) are given by the inner products of row (*e*) with the rows of the correlation table.

The step by which row (*g*) is obtained from row (*f*) is a little more complicated than the corresponding step in the calculations for the first factor loadings. From each number in row (*f*) we subtract not only the corresponding trial loading in factor II but also a correction which eliminates the effect of factor I; this correction consists of the corresponding number in row (*d*) multiplied by $-0{\cdot}0234$, the inner product of rows (*e*) and (*d*). Thus, for example, the number 0·177 in row (*g*) is equal to

$$0{\cdot}330 - 0{\cdot}170 - 0{\cdot}727 \times (-0{\cdot}0234).$$

In general, where more than two factors are assumed to be present and where further approximations are being calculated for the loadings in the rth factor, there will be $(r-1)$ such corrections to be subtracted, one for each of the preceding factors.

Having found row (*g*) the quantity k_1^2 is now given by the inner product of rows (*f*) and (*g*), from which, taking the square root of the reciprocal, we derive $1/k_1$. Row (*h*) is then obtained by multiplying the figures in row (*g*) by $1/k_1$, or 0·9500. We have thus found second approximations to the loadings in factor II.

The whole process described above may now be repeated over and over again until the required degree of approximation is reached. In general very great accuracy is not essential, and the final estimates of the factor loadings and specific variances given in Table 5 are not necessarily correct as regards the third decimal place. They are, however, sufficiently accurate for our purposes.[1]

Table 5

	Tests							
Loading in	1	2	3	4	5	6	7	8
Factor I	0·725	0·503	0·664	0·661	0·623	0·399	0·726	0·694
Factor II	0·172	−0·261	−0·468	0·087	0·069	0·027	0·106	0·291
Specific variance	0·445	0·679	0·340	0·556	0·607	0·840	0·462	0·434

II. Testing significance

Taking the above estimates of the factor loadings we are able to calculate in the usual way the residual correlations left after taking out the effect of the two factors. These residual correlations are given in Table 6. It will be noted that in the diagonal cells of this table we have placed the specific variances.

Table 6

	1	2	3	4	5	6	7	8
1	(0·445)	−0·008	0·004	−0·037	0·036	0·056	−0·024	0·011
2	−0·008	(0·679)	0·004	0·006	−0·016	−0·021	0·001	0·015
3	0·004	0·004	(0·340)	−0·004	−0·001	0·006	0·001	−0·002
4	−0·037	0·006	−0·004	(0·556)	0·042	−0·044	0·027	0·002
5	0·036	−0·016	−0·001	0·042	(0·607)	−0·011	−0·019	−0·035
6	0·056	−0·021	0·006	−0·044	−0·011	(0·840)	0·009	−0·023
7	−0·024	0·001	0·001	0·027	−0·019	0·009	(0·462)	0·012
8	0·011	0·015	−0·002	0·002	−0·035	−0·023	0·012	(0·434)

A convenient measure may now be found for deciding how well the data fit our original hypothesis concerning factors. To calculate this measure, which we shall denote by w, we square each of the above residual correlations and divide by the product of

[1] The factors may of course be rotated as desired.

the numbers in the corresponding pair of diagonal cells. Thus, for example, the residual correlation for tests 4 and 7 would be squared and divided by the product of the fourth and seventh diagonal elements, giving the result

$$\frac{(0{\cdot}027)^2}{0{\cdot}556 \times 0{\cdot}462} = 0{\cdot}002838.$$

There are altogether 28 such terms, one for each residual correlation, and w is obtained simply by forming the sum of these terms and multiplying it by 443, the number of children in the sample. The result is found to be 20·1.

In order to decide whether this value can be regarded as significant we use a χ^2 table such as is given by Fisher & Yates(4). All that is necessary in using it is to determine first the number of 'degrees of freedom'. In general this number is given by

$$\tfrac{1}{2}\{(n-m)^2 - n - m\},$$

where n is the number of tests and m is the number of factors assumed. In our case, putting $n=8$ and $m=2$, it will be seen that the number of degrees of freedom is 13.

Entering the χ^2 table with 13 degrees of freedom we find that the 1% significance level is 27·7. This means that if our hypothesis that only two general factors are present is correct, then the chances of getting a value of w greater than 27·7 is only 1 in 100. If, therefore, we had obtained a value of w greater than 27·7 we should have been justified in rejecting the above hypothesis and in assuming the existence of more than two general factors. In our case, however, the value of w is only 20·1, well below the 1% significance level. We have thus no grounds for rejection, and although we cannot state that only two general factors are present, we have no reason to assume the existence of more than two.

The only condition in using the above test of significance is that N, the number of individuals in the sample, should be fairly large. The foregoing methods are of course applicable whatever the number of tests administered and whatever the number of factors assumed.

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SOME BIOLOGICAL ASPECTS OF THINKING

By A. W. WOLTERS

A few years ago (1) I ventured to suggest that it might be well to reconsider the traditional treatment of thought-psychology, so far as it had grown up in dependence upon laboratory research into the phenomenology of the process. The classical work of that school, as of earlier writers, was an attempt to describe thought in terms of content. It may be doubted whether the results justify the expenditure of labour. Industrious research has accumulated a vast mass of interesting and important material, but none of it seems really to go to the root of the matter. It is to be remarked that those who require a theory of thinking which they can apply in practice, the philosopher, the educationist, the therapist, always ignore the results of these psychological researches into phenomenology, and rightly so, for they will find nothing there which can be put into service. I then urged that it would be well to turn to the process itself, and more particularly that the ancient controversies regarding universal or general thinking might be put to rest so far as psychology is concerned if it were accepted that generality was to be sought in the manner of the thinking, and not in introspectible contents. Putting the matter a little differently, I urged that if psychology is to use the term 'concept', then we must regard the concept as implicit in the mental behaviour, as of the nature of a skill in reacting rather than as an entity. But to say that concepts are skills is to bring conceptual thinking into line with many other processes. This is the aspect which I wish to discuss here. I wish to offer a further defence of my hypothesis, and to show more adequately than before how thinking falls into line with other forms of behaviour.

I am obstinately convinced that psychology is the study of human beings considered as behaving organisms. I do not readily accept a psychology of *thought*; the subject-matter of our science is surely the person thinking. This is no doubt a glimpse of the obvious, but it is all too easy to overlook it. My approach, then, to psychology is frankly biological. It may fairly be questioned whether this is the right attitude to adopt towards thinking, but unless the biological standpoint is adequate when working in the department it must be abandoned everywhere, since we cannot tolerate a hypothesis which cannot be applied to all levels of psychological process.

We may take self-preservation as the cardinal function of organisms, since all others are dependent upon the successful discharge of this. Once again it is a commonplace to remark that the primary end of an organism's activities is to maintain itself in being, but it is one that is often insufficiently developed. Let me throw out a challenge by offering a working definition of psychology as a study of organisms struggling to remain alive. It is from that root that we have to develop the psychology of thinking. The general process of self-maintenance is commonly described as one of adaptation to environment, a phrase which does something less than justice even to the lower organisms, for they, and all animals in their struggle for survival, attempt to control their environment. Their rise in the scale is essentially an advance in their powers of exercising sovereignty over the world. As simple examples I may mention the hermit-crab and the

caddis-grub. The most common way of adapting the environment is to eat part of it. Our definition may now be a little modified. Our science is the study of the behaviour of organisms engaged in securing control over their environment to the end that they may continue to live.

There are cases, however, in which the animal adapts itself to circumstances in a most literal sense, as in the serpent's gape when swallowing prey, and the lizard's self-amputation of the tail as a means of escape. The lobster has claws of different size, the larger employed for aggression, the smaller for holding and cutting. If it loses the smaller it grows a new one; but if the larger is lost the smaller grows up into a fighting claw, and it develops a new smaller one, thus changing hands as it were. Many of the Turbellaria, Mollusca and Protozoa add to their defences by swallowing nettle-cells, which are neither digested nor excreted, but passed through the tissues of the animal to the surface where they remain alive and active. I wish to avoid any controversies regarding vitalism, which facts like these excite, and I think the facts are stated with a minimum of interpretation if we say that the organism employs itself as an instrument for its own preservation. I admit that I have selected the most picturesque examples of this process, but only to show up the more plainly what is common enough at all levels. Consider, then, a variation of this theme, a dog licking a wounded paw. The odd feature of this is that the animal is behaving towards itself as it behaves towards other objects. Agent, instrument and object are one. There is reaction to that which reacts. The organism has become a part of its own environment, to be taken into account and controlled as the rest of the universe is controlled.

One form of self-modification is learning by experience. I call learning a modification of the organism because the profit which it derives from its experience cannot be separated, even in thought, from the animal itself. The only observable fact is that its behaviour is different at this epoch from what it was at an earlier. The only direct inference which we are entitled to make is to relate systematically the present reactions to events of an earlier date. If those events had not been *so*, these reactions would not be *thus*. It may be that a future psychology will rest content with statements of this kind, but at present there is an inveterate prejudice against action at a distance, as much in time as in space. The human mind cannot yet believe that yesterday's madness this day's did prepare, unless it can provide that madness with lodging overnight. We seem driven to postulate something permanent or continuing to relate 'then' with 'now'. But the only thing which can bridge the gulf here is the organism. Reactions are observed to have changed, while the circumstances evoking them remain sufficiently constant. We consider that in any case the organism contributes something to the character of the reaction. Now we regard it as having been changed in character by its previous reactions, and being to that extent another animal, it behaves differently. We are reluctant to ascribe explicit memory to the lower organisms, and we know that human learning need not entail representational memory. We must then regard the results of earlier experience as being something which the behaving organism now is, not as something which it has. It has been modified, and has to be described differently.

Let me summarize this preliminary survey. The starting point was the tendency of organisms to persist in being. They achieve this end by developing control over their environment, by employing themselves as instruments, and reacting to themselves. They are modified by their own behaviour, and become more efficient in the process. This

should be a sufficient battery for an attack upon the problem of thinking, except that one of the weapons stands in need of improvement.

Learning by experience, if it is to be really profitable, must do something more than leave the subject more ready to carry out a useful reaction again, when the circumstances are repeated, because circumstances so seldom repeat themselves in complete detail. A tendency to exact reproduction of activities is therefore disadvantageous. Actually we find that animals are nearly always able to effect some degree of modification to meet the altered requirements. They have acquired a reaction tendency which can be exactly re-determined by the particularities of the situation presented to them. They face the world with a preformed readiness to behave appropriately to any one of a limited range of alternatives. Their acquired tendency is open and flexible. Such behaviour, in part predetermined, in part variable, I term schematic, and the constant pattern of the reaction I call a schema of behaviour. We can only observe the schema in the actual behaviour, but in virtue of the propensity mentioned above we are apparently compelled to infer from it that some permanent system of conditions exists within the organism. I have said that we must regard the organism itself as modified when reaction tendencies were acquired. Then since the schematic character applies more properly to the predisposition than the particular piece of behaviour, we should regard the schemata as acquired characters, as modifications of the animal.

It will be remembered that Prof. Bartlett has also used the term *schemata*. He says: "'Schema' refers to an active organization of past reactions, or of past experiences, which must always be supposed to be operating in any well-adapted organic response.... There is not the slightest reason, however, to suppose that each set of incoming impulses, each new group of experiences, persists as an isolated member of some passive patchwork. They have to be regarded as constituents of living, momentary settings belonging to the organism, or to whatever parts of the organism are concerned in making a response of a given kind..." ((2), p. 201). With this I am quite in agreement, but I wish to insist that this setting retains commonly some degree of adaptability.

It was reflexion upon this blend of determination and variability in animal behaviour which led to my view of conceptual thinking, for it seemed unavoidable that we should recognize here the presence of what must be called universals. In what other terms should we describe the behaviour of those famous hens selecting consistently the darker of two surfaces? We are disinclined to ascribe explicit thought processes to hens, nor is it necessary, since much of our own behaviour is of the same kind as theirs. Thus I was led to conclude that the most promising line of psychological theorizing about this matter was to regard the universal as being primarily a way of thinking, and not an object of thought. Then, since I consider thinking to be a species of behaviour, I described universals, or concepts, as manners of behaving.

Having at last reached the higher levels of behaviour we can find more apposite examples of schemata. Perhaps the best is found in the grammatical structure of off-hand speech. Our words fall into due order without deliberate intention. Few persons of my age could give any clear account of the grammar of their native tongue, and even those better informed ones would be unable to converse with any ease if they thought of grammar while speaking. But there is a high degree of constancy of pattern, and the pattern controls the sequence of words as they are uttered, though the uniformity is not reached by repetition of words or phrases, since these may be largely new. The organiza-

tion is something living and flexible. It is unfortunately difficult to devise an entirely original statement to illustrate my point, and all that I could achieve when constructing this paper was to say that "to the best of my belief I have never before remarked, nor, in my experience, has it ever been observed by others that the hippopotamus, though of sturdy frame and robust constitution, is not seen to the best advantage when employed in drawing a sledge in the course of polar exploration". I believe my nonsense will parse and analyse correctly, although I took no heed of the grammar as I composed it. Between the two full-stops are seventy-nine words, in a combination which has probably never occurred before. It is not a habit of mine to use sentences of that length, so that the structure is to some extent new. I can only place the facts by falling back upon my earlier formulation. As the result of experience I have become modified so as to be prepared to construct sentences of an accepted pattern without thinking of the task. It is not repetition, but control. So a person's ability to speak grammatically provides an excellent model of a higher level schema, though, of course, not a conceptual one. Another large-scale example is presented by the behaviour commonly classed as business habits. Here again there is a certain constancy of pattern with continual variation of the detailed activities. Now we can bring into the same class all skills, all techniques which have been thoroughly acquired. Experience continually hardens them, and yet leaves them flexible. They become at once more determinate and more varied in their functions. These are the weapons with which we seek to bring the world beneath our sway. But they are not weapons which we need to apply by deliberate effort. For the most part they come into action when the need arises. In Bartlett's phrase, they are "that with which we react". So a concept applicable to the lower animals will cover much of the highest forms of human behaviour.

If this be accepted it becomes difficult to formulate any decisive differences between thinking and other activities. In one respect, and that the most important, they are clearly alike. Thinking is an adaptive response to a situation. This is not to say that I fail to recognize important differences between higher and lower levels of reaction, but I consider that the differences are not psychologically so fundamental that thought should be marked off as a unique and altogether mysterious sphere. The differences are in the nature of the problems, the material employed for their solution, and the schemata involved. These are important enough, but in no way decisive for theory. One man travels westward by train to sell his house, another rides eastward on a camel to buy a tent. The very complete difference in the details of the actions does not prevent the recognition of a common function, the utilization of available means of transport for the control of space in achieving an end. So there may be identity of psychological function in the responses to qualitatively dissimilar situations. In fact the differences between thinking and other processes are only to be stated in rough and ready terms when the psychology of the matter is under review. The person thinking displays many of the characteristics of the lower animals. When confronted by an intellectual difficulty he may evade it or give it up, just as animals evade or submit. We may be more aggressive and conquer the difficulty even at the expense of dealing brutally with the relevant facts, or we may accept the situation and modify our behaviour in accommodation to it. I had the clue to this in my hands years ago, if I had been alert enough to recognize it. Trying to discover the psychological differences between affirmation and negation, I found that the data would not fit into the conventional logical framework. The processes, which

were certainly thinking, fell into three classes, which were very reminiscent of simpler forms of behaviour. There was the laborious search for a result, like a dog digging out a rabbit, gratified acceptance (a kind of mental nodding of the head) or hostile, mildly angry rejection. What attracted my attention was not so much the formal similarity of the process of judging to other forms of activity as the picture of the whole setting which the reports presented, the strained and anxious expectation of the problem to be presented, the triumph and relief, suspicion and hostility. The great differences in the amount of conscious material utilized in the reactions were intelligible when considered in relation to the problem and what I knew or learned of the subjects. Often a statement was confidently denied although the subject could not report what he had denied; he only knew that it was wrong. In such cases the organized schemata govern the response without the mediation of conscious processes (4). But we can make the relation of thinking to the more primitive reactions more definite than this by going back to the characters which we discovered earlier.

Are we to regard thinking as having for its primary function self-maintenance? So far as the earliest and simplest forms of thinking are concerned there can be no doubt that we must. For this thinking is only preparatory to dealing with material things, and a substitute for such activities. But can the view be extended to the sphere of purely speculative or theoretical thought? I consider that there is no doubt that all thinking response can be, and should be, regarded as directed to preserving or extending the integrity of the organism. Lest it should seem nonsense to suggest that a mistake in metaphysical speculation should be regarded as a danger, let me offer one or two instances to show that events which are in no sense material may lead to disastrous consequences. It is admittedly dangerous to mistake an arsenical solution for Chartreuse. Is it any less dangerous to think that two and two are five? If you will contemplate in some detail an imaginary life conducted throughout under the influence of that belief, I am sure that you would conclude that it would be better to mix your drinks. It is a calamity to lose permanently the use of a limb. It would be worse to forget for ever your name. These are admittedly gross cases, but they indicate that every failure to make the correct reaction, even in the ideational region, is fraught with danger. The organism seeking to preserve its integrity has more than merely physical characters. It has to preserve its enlarged capacity for action also. Its dangers have increased with its possessions, and it has to meet the new difficulties arising from its social and mental environment. It has met them hitherto by a continuous process of growth, that is of modification of itself, as we saw the lower organisms did, though the steps may be more obvious and dramatic in the case of the latter. For the preparation for future behaviour, I must repeat, is a character of the organism, not a machine to which it can have recourse.

Our intellectual behaviour is as dependent upon schematic preparations or flexible skills as the muscular behaviour of animals. They are only revealed in the behaviour, and operate without necessary representation in consciousness. I have instanced elsewhere our use of numbers to illustrate how a skill may be highly developed without any corresponding development of the power to think about the skill (3). The skill, and this skill deserves the name of concept if anything does, is developed to meet problems of one kind, and that kind is not concerned with questions about its own nature. Raise that, and you have started a new hare, to be pursued by a fresh pack. In their primary characters concepts are not objects of attention but patterns of behaviour. Animals

remain at this level, and young children seldom get far from it. But it is true of all of us that our latest and highest schemata in any department are of this instrumental kind.

But, you will say, there comes a point at which we must part company with the lower organisms. Our most trusted schemata have a way of failing us, sometimes when the variation of detail is too great, or when a new type of problem arises concerning it, as may happen in conversation. Social settings give rise to many situations of this kind. Then, in Prof. Bartlett's phrase, the organism has to turn round upon its schemata. But I suggest that lest we suppose that schemata are entities which the organism has, it is well to remind ourselves once more that these schemata are acquired characteristics of the organism, in no way, and in no degree, separable therefrom. Therefore, the organism is really turning round upon itself as now modified by its racial or individual past. To refit a schema means deliberate self-modification. The thinker is now reacting to a part or aspect of himself. But this is not new in principle, for we saw the wounded dog doing it. Nothing is much more common than reaction to self. So once more a process which represents the highest intellectual achievement of man has its roots in behaviour manifested very low in the scale. I am not speaking in metaphors or analogies. I wish to be understood literally when I say that scrutiny of one's ideas is a reaction to self developed from animal behaviour, and directed to essentially the same ends, the removal of a felt uneasiness arising within the creature, or reshaping oneself to crawl through, round or over an obstacle. Further, man and dog are under the necessity of reaching the part affected, if they are to do anything about it. The schema has to be made accessible to the thinker, and here the various studies of phenomenology have their value, a value perhaps greater in the theoretical context which my views will give them. But let us once for all abandon the mythology of ideal entities.

Possibly you feel prepared to go some part of the way with me but not all. You are perhaps ready to accept my insistence upon the existence of determinable schemata in animal behaviour, even to the extent of considering that I am proclaiming the commonplace. You may be willing to admit that very much of human behaviour is of the same kind, and yet not be willing to admit that all the concepts or ideas of which we so often speak, are at bottom only reaction patterns. You will say that I have slid too easily over the fact that ideas appear before consciousness. Of course, when I have grouped buttercups together, and perhaps learned to call them by a common name, I can then think of my way of regarding them, mentally manipulating my experience of them by means of any bits of imagery, attitude or what not. I am preparing for further mental behaviour, and I only undertake the task because a question is presented to me, a difficulty to which I have to adjust myself. I have hinted at my way of dealing with problems arising from classificatory concepts; the difficulties with them are of detail only. A much more formidable problem arises regarding abstract ideas. Are the great normative ideas after all only forms of behaviour capable of being brought into line with animal behaviour, not of course in detail, or value, but in psychological character? When it is said, as so often to-day, that the idea of political liberty is ceasing to rule men's actions, what, as psychologists, are we to understand by that idea?

If you still hanker after the belief that an idea is some kind of entity which the mind contemplates, you must be prepared to answer one or two elementary questions. Where does the idea of liberty exist? Psychologists will not ascribe them to Plato's heaven, if only because the ideas which we encounter are so very mutable, and in no wise eternal.

But when we speak of an idea being current in society, while we have to ascribe it to thinking humans, we surely mean more than that it is present in a certain mind, or even in a number of such minds. Imagine a person confronting, in actuality or report, a community who value the idea of freedom, even to the extent of discussing it. Our subject can only become aware of the idea through the behaviour in word and deed of the members of the society. He observes that they act toward each other in certain regular ways, and that the well-being of any person in that district is dependent upon acting in those ways, for of those only do the people approve. A stranger would discover how to behave probably by trial and error, as the hens learned to select the grey, and like them he would develop an organized system of directed but variable response tendencies. So he might develop patterns of speech behaviour pleasing to his fellows, and do all this without ever reflecting upon the meaning of the word *liberty*, without, as the moralist would say, understanding liberty at all. This is not cynicism; just a simple description of the attitude of most men towards their highest values. Many have died for liberty who could not have put into words the ideal they were defending, who, indeed, had never thought about it. For in its first psychological function the idea of liberty is a way of living, and not an object of contemplation. The stability of political parties is perhaps dependent upon the fact that the ordinary member thinks *with* his political ideas, and not about them.

Let us next suppose that our observer is interested in theories, which means that he finds himself troubled by problems presented by the form of the citizen's behaviour. He now has to master a wider universe if he is to retain his mental integrity and be at peace. He studies the pattern of men's behaviour, tries to trace its growth, and to predict consequences. He will be interested in their attempts to describe the motives which lead to their actions, but their words will have no meaning to him unless he can bring schemata to bear upon them. Dictionaries will not help him; he must be shaped to understand. He is now attempting to adjust himself to a formidably complicated fact, which is threatening unless he can master it. The fact is the complicated network of muscular and verbal behaviour displayed by individuals in society. So he has to turn round upon himself and deliberately make his own reaction patterns visible to himself, reconstruct them, and fit them to the problem set by social activities. He will blunder, lose the scent, and cast about experimentally for a long time before he is able to envisage the principles of what he sees and hears, and to understand how those most subtle and comprehensive schemata which we call ideals dominate action. In the end he will only obtain a sketch of all that the word 'liberty' denotes. There are limits to our powers of envisaging the structural principles of our thinking, and to think about thinking involves the construction of a new instrument, and that takes time. It may be that many philosophical and scientific difficulties are due to the fact that man, having developed his behaviour under the stress of immediate material needs, is only lately forming reaction patterns in relation to problems of a different kind. I suggest that there are problems about which man cannot yet think with any usefulness, not so much through lack of data, as because he has not yet forged the necessary mental tools. In the instance under consideration the difficulty arises from the complexity of the facts. The idea of liberty is primarily the pattern of behaviour of social individuals in relation to each other. It can be apprehended in some degree, but fundamentally it is an orientation to other people. When now we turn to the question we took for examination, the background to

the statement is a frustrated expectation. Our prepared behaviour does not work; the mental set is disturbed. We resolve the conflict partially by examining our own schemata, and finding a verbal formulation which will denote the situation we have to meet in our dealings with members of the other communities.

So all the great ideas exist in the behaviour of individuals acting in relation to each other, so that the pattern transcends the merely individual. There is no need for mysticism: the psychologist has not to deal with ideas, but with people acting.

I believe then, that the route to a useful psychology of thought is by way of behaviour, and not by way of ideas, if we mean by idea a mental entity, or content, something before or in the mind. I stress the conative aspect of thinking, regarding it as a development of the cardinal function of self-preservation. So far as it involves representative or symbolic conscious processes, thinking is an extension of the development which has given us distance receptors and an elaborate musculature. The most important part of my hypothesis is the partial reduction of the process to the operation of those organizations which I have called schemata. The effect of these two principles is to bring back thinking into the whole web of behaviour. For the range of schemata is very wide, covering probably all human reaction, cognitive as well as conative. They are operative in the simple act of perception as much as in an elaborate practical activity, they are converged and interweave in all skills, in language, sentiments and instincts. So behind any process of thinking stands the personality of the thinker. It is surely a gain to see thought no longer as a peculiar and highly specific kind of response to be dealt with either in terms of mysticism or mechanism. It is true that we have lost our 'ideas', but psychology can travel the lighter in consequence. I do not pretend that we have solved the mystery of thinking, but we have shifted its location. It has become one aspect of the most profound of all scientific mysteries, that of being and remaining alive.

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PUBLICATIONS RECENTLY RECEIVED

Seasonal Influence on Growth, Function and Inheritance. By A. B. Fitt. New Zealand Council for Educational Research, Educational Research Series No. 17. London: Humphrey Milford. 1942. Pp. xi+182. 10*s.* 6*d.* net.

Prof. Fitt has produced a monograph of the greatest interest to all educators, psychologists and social workers. He has surveyed, analysed and expounded with great clarity an immense amount of work dealing with seasonal variations in a large number of physiological and mental functions. He includes studies of variations in weight, height, memory and related functions, intelligence, certain learning processes, muscular capacity, resistance to disease, delinquency, and a number of other types of behaviour. He has considered the inheritance of most of these functions in relation to the month of conception, and he arrives at some extremely important conclusions. His picture is one of a human organism subjected to broadly different kinds of strain during an autumn-winter period as compared with a summer-spring period. The major stresses of modern life come in the spring and the summer, but whether as a result of this, or for some other reason, the major developments in many cases come in the autumn and winter. Recognition of this, he strongly and correctly argues, is required for the interpretation of any study of the concomitant development of physical and mental functions. Further, he asserts, "the work-year should be adjusted to the seasonal rhythms. Periods of heavy mental work ought to fall in the autumn and the winter, with examinations not later than December in the Northern Hemisphere and June in the Southern Hemisphere. Again large-scale testing and measuring of human functions ought to take account of all well-attested seasonal variations".

These general remarks will be sufficient to indicate the importance and interest of Prof. Fitt's well-documented and acute study. For details the book itself must be consulted, and it is a pity that the monograph could not have been produced more cheaply, so that far more students could have had a reasonable chance to profit from a piece of research which was well-conceived, well-carried out, and which must remain of wide general interest for a long time to come.

Social Learning and Imitation. By Neal E. Miller and John Dollard. Published for the Institute of Human Relations. New Haven: Yale University Press. (London: Humphrey Milford.) 1941. Pp. xiv+341. 21*s.* 6*d.* net.

This book begins with a detailed discussion of the principles and conditions of learning, and then proceeds to study the particular case of learning by imitation and copying, illustrating the study by experiments on the learning of rats and of children by imitation. Such learning is especially important in social situations, and the social settings in which it normally occurs are discussed, and the type of model most frequently imitated. In the light of the principles laid down in this study, crowd behaviour (including a peculiarly revolting case of negro lynching) is analysed, and the diffusion of culture. The book is perhaps somewhat unnecessarily lengthy and heavy-handed in its analysis of the imitative processes; the reader may sometimes find it a little difficult to see the wood for the trees. But it is valuable that the immense importance of social imitation should be stressed—if only as an awful warning of the difficulty any individual must find in breaking away from its limitations and deviating in any way from the courses of action it inculcates.

Backwardness in the Basic Subjects. By F. J. Schonell. Edinburgh: Oliver and Boyd. 1942. Pp. xix+560. Price 18*s.*

This book is certainly one of the best so far produced by a British educational psychologist. The title is, perhaps, a little misleading, since it suggests that all the basic subjects are covered, whereas arithmetic and writing are both omitted. But the treatment of reading, spelling and composition is comprehensive, very thoroughly documented, and soundly based upon experimental investigations. Moreover the book is written in so practical and sympathetic a manner as to be of immediate use to teachers of backward children, or indeed of normal children too.

From more than 15,000 children in good, medium, and poor elementary schools, all those were picked out for intensive study who were 1½ or more years backward in one or more specific subjects, but who were normal or nearly so in other subjects. Some 5½% of boys and 2½% of girls fell in this category, the proportions naturally being larger in schools of poorer socio-economic status. Standardized attainment tests (many of which are published for the first time in the Appendices to the book) were used for the classification and for diagnosis of the nature of the backwardness. The discussion emphasizes, much as does Burt's *The Backward Child*, the multiplicity of causes—intellectual, emotional and environmental—and goes on to give detailed suggestions and exercises for remedial teaching of English. It is hardly possible in a short review to do justice to the wealth of contents of the book, but the psychologist will find particularly interesting the account of the development of ability in composition among 7–13-year-old children.

P. E. V.

Literature for Individual Education. By ESTHER RAUSHENBUSH. Sarah Lawrence College Publications: No. 1. New York: Columbia University Press. (London: Humphrey Milford.) 1942. Pp. xiv+262. 18*s.* 6*d.* net.

Although it is not specifically stated, it would appear that Sarah Lawrence College, U.S.A., is intended for girl pupils of the age of about 16 upwards for four or five years. Its watchword of 'individual education' means that a great amount of attention is paid to individual differences, that fixed curricula are avoided as far as possible, and that pupils will learn, or be given the chance to learn, more or less on their own responsibility, how to find their way about in whatever subjects they elect to study. This certainly does not mean that co-operative activities are banned, or even discouraged, but that, so far as academic work goes, they are largely pursued by a method of fairly free discussion. It is perhaps inevitable that the method of exposition is very largely concerned with particular cases and illustrations, so that it is less easy than might be desired to see how the plan as a whole works out in group life. However, abundant details are given of how the scheme has developed in connexion with studies in literature. No doubt the main differences between this and more conventional lines of approach must lie in an 'atmosphere' which is very hard to convey, for so far as the ground covered goes there does not seem to be a very great amount of difference, except in the very much greater preponderance in 'individual education' of contemporary writings and particularly of contemporary novels. Indeed much of the book suggests that what is chiefly accomplished is to give students more guidance than is usual about what in the ordinary way would be their off-time, or leisure time, reading. Guidance there certainly would appear to be, and quite a lot of it. The author writes as an enthusiast, and nobody can doubt that many students would find the methods used vastly more attractive than those involving the very fixed syllabus and the assumption that there is 'one best way' in education. As to the results of the system, these are not very clear. Two cases are followed up for four complete years, but it is not clear how typical they are. Very much more interesting will be a follow-up study continuing throughout several years of post-institutional life, and it is to be hoped that this will be done as opportunity offers. Meanwhile the book, though rather unnecessarily long and very unnecessarily expensive, has much to attract the psychologist interested in adolescent education.

Educational Psychology. By A. I. GATES, A. T. JERSILD, T. R. MCCONNELL and R. C. CHELLMAN. New York: The Macmillan Company (London: Macmillan and Co.). 1942. Pp. xviii+805. 16*s.* net.

Although this volume is regarded as a revision of Professor Gates's *Psychology for Students of Education* it is to all intents and purposes a new text. It has been greatly extended and very carefully redesigned. It remains essentially a general text-book intended to 'offer material which will help the teacher to see his professional activities with deeper insight and in broader relationships and to carry forward his work with more confidence and satisfaction'. It is a notable advance that the authors discard the view that they should confine their attention to those fields within which conventionally complete experimental data are available. They are chiefly guided by the actual development of aims and methods in humane education. The higher mental processes, for example, get more attention than the far more fully studied problems of rote learning. The discussion is sober and critical throughout, not at all exciting to read, but very thorough. Adequate references are given. As a text-book this deserves and should achieve success.

A History of Medical Psychology. By GREGORY ZILBOORG, in collaboration with GEORGE W. HENRY. London: George Allen and Unwin, Ltd. 1942. Pp. 606. 28*s.* net.

This is a book which should be in every medical and psychological library. There is no other volume which covers anything like as extensive a field of psychological medicine as is covered by this one, or that discusses its subject matter in so clear and attractive a manner. Moreover, any non-medical psychologist with a reasonable amount of knowledge can read it with profit and sustained interest. Beginning with primitive and oriental medical psychologies it traces the development of thought through the Greek and Roman periods, through the dark ages that followed, with their surrender to magic, demonology, and what the author calls 'the blows of the witches' hammer', through the brightening of the dawn of genuine observation and research towards the end of the sixteenth century and right down to modern times. Particularly interesting is the story of how the long reconstruction age which followed the re-birth of learning eventually hardened into an 'Age of Systems', to be followed once again by a psychiatric revolution headed by Freud. Two final and very valuable chapters dealing with Organic Mental Diseases and Mental Hospitals are written by Dr Henry. The volume is a record of wide and accurate learning modestly and attractively presented. It is adorned by a number of excellent illustrations.

Diseases of the Nervous System. By F. M. R. Walshe. Edinburgh: E. and S. Livingstone. 1940. Pp. xiii + 288. 12*s.* 6*d.* net.

This is a text-book intended for use by the general practitioner and the medical student. It is concise, lucid throughout, and well arranged. It is in general not concerned more than is necessary with anatomical detail, and as far as is possible Dr Walshe avoids all use of 'complex and redundant' teminology. His aim is to write a narrative of the forms of disease with which he deals, and in this aim he succeeds to a remarkable extent. The book in consequence, though it contains no case histories, and though all its description remains in truly general terms, yet retains a liveliness and interest often lacking in text-books on this subject. The treatment of the psychoneuroses is admittedly somewhat inadequate, and from the point of view of the practitioner or medical student who is interested in the psychological side of medicine this is a defect which it is to be hoped will be remedied in future editions. At the same time there remains a great amount of material to interest the psychologically minded reader, and the book deserves wide and lasting success.

A Survey of the Literature on Artistic Behaviour in the Abnormal. By Anna Anastasi and John P. Foley, Jnr. Annals of the New York Academy of Sciences, Vol. xlii, Art. 1. Pp. 1–112. 1941.

This monograph, styled 'Approaches and Interrelationships' is concerned with the artist's attitude toward insane art, not with any relationship of the latter to modern art generally. 'Art' seems to be used to include any sort of drawing and other art form, good, bad or indifferent, representative or fantastic. An extensive literature is described in some detail.

Three types of approach to insane art products are distinguished: The artistic, the psychiatric and the psychological. The artistic concerns alleged aesthetic values. Some artists extol the products of the insane and find them a source of inspiration, others deplore the tendency to applaud them. The psychiatric approach is concerned with diagnostic and therapeutic values. As regards the first there is some agreement, but a great amount of disagreement: 'Conclusions in this field are difficult to formulate with finality due to taxonomic differences and to lack of adequate empiric control'. As regards the second, while positively favourable results are often reported, no exactitude of general statement is possible at present. The psychological approach is concerned with the mental functions, their nature and grade, exhibited by the insane artist. Here again the amount written is vastly out of proportion to its definiteness.

An account is given of the large number of parallels which have been drawn between insane art, and the products of primitive man, of children, of folk productions, of special art movements and so on. It appears that while there are many resemblances, there are also many differences. The literature dealing with genius and insanity is also reviewed, and is shown to be predominantly speculative. Such few quantitative studies as there have been dealing with the relationship between personality traits and artistic ability have so far failed to reveal the artist as a distinct personality type.

The monograph casts a wide net, and is by no means always limited to the topic of its title. It is a workman-like survey but leaves a strong impression that an enormous amount of work has resulted in very little by way of clear issues.

A considerable bibliography is appended.

Foundations for a Science of Personality. By Andras Angyal. New York: The Commonwealth Fund (London: Humphrey Milford). 1941. Pp. xii + 398. 13*s.* 6*d.* net.

The philosophically minded psychologist will probably read this book with considerable interest. Others are likely to find it intolerably general, and to discover a very great difficulty in determining whether foundations with no superstructure to speak of are well and truly laid or not. The author is a medical doctor, resident director of research at Worcester State Hospital, Massachusetts. It seems therefore all the more of a pity that he does not support his wide theorizing and use of sweeping terms by an adequate array of the facts which beyond a doubt he must know. The book is in the line of a great many modern discussions which regard the whole as vastly more important than its parts. Its philosophy is holistic, its psychology *Gestalt*. Personality is defined dynamically, as the process of living, and this process is asserted to have the two tendencies of 'the achievement of greater self-determination (trend towards increased autonomy), and the attempt to reach greater conformity with society (trend towards homonomy)'. These two trends develop a dynamic pattern, the key to which lies in processes of integration and organization. The rest of the argument, which is all carefully worked out, is all on the same level of generality. One is left with the impression that a lot of extremely difficult language is being used to describe processes, facts and events which hardly seem to call for elaborate disguise.

Psychorama. A Mental Outlook and Analysis. By A. A. Roback. Cambridge, Mass.: Sci.-Art Publishers. 1942. Pp. 365. $2.90.

'Psychorama' is said to be a term meant to indicate or provide a description of 'the particular mental process or sequence which relates historical events to one another, with a view to forecasting the future in the light of human motives'. It is contrasted with 'politorama', an outlook characteristic of modern statesmen in general and of the late Mr Neville Chamberlain in particular. This appears to mean a habit of looking only or mainly at particular political events without any attempt at all to estimate their probable remote repercussions. However, on reading the book it becomes clear that the name does not matter very much and that in fact 'psychorama' stands for anything that Professor Roback likes to take up, in any style of writing that pleases him at the time. The book is made up of a large number of exceedingly short articles, most, but not all, of them reprinted from a variety of magazines and journals. They are collected loosely under the headings 'Issues', 'Books', 'Men', 'Events' and 'Grins and Groans'. Most of them deal with contemporary interests and the bulk of them have to do with Nazi ideas or Nazi personalities or Nazi practices, to all of which Professor Roback is gloriously and uncompromisingly antagonistic. Very nearly all the *Pro* in the book is Jew, and very nearly all the *Anti* is Nazi, except for a few scourgings of Mr Neville Chamberlain.

Any psychologist is likely to find this a most interesting, amusing and stimulating volume to pick up and dip into at odd moments. The articles are extremely unequal: some of them are just bad jokes, others very good jokes, and a few—especially the three papers about Jung—contain some penetrating psychology. Very likely only a few people will honestly think that the substance of the book warrants its size or its cost. It is, however, very clear that Professor Roback has thoroughly enjoyed the writing of it, that he ranges easily over a vast country of problems, that he has the most decided opinions about current history in all its many fields, and that—perhaps with some lapses—he brings to his tasks, as most readers who are familiar with his other writings would expect, a very lively, honest and decisive psychological intelligence.

Basic Concepts in Social Case Work. By Herbert H. Aptekar. The University of North Carolina Press. (London: Humphrey Milford.) 1942. Pp. ix+201. 15*s.* 6*d.* net.

A most interesting problem is presented by the analysis of the psychological processes at work and the relationships created between the social case worker and his client. The interviews which take place between social workers and applicants for relief resemble in some ways the medical consultation and the psychiatric interview. But they also have their special characteristics, chief of which is the marked ambivalence of the client who both wishes to obtain help and monetary relief, and at the same time is ashamed of doing so because of the social stimga he feels it involves. The author rightly stresses this factor of ambivalence and the very marked resistance it is liable to set up to accepting the very help which is desired. Moreover, the social worker, although he must attend principally to the immediate objective facts of the situation and of the client's appeal, will not make the best of the interview unless he is alive also to the underlying factors of the situation—the factors in the client's personality and experience which have brought him to seek relief. Much of the value of the interview depends on its cathartic properties (excellently illustrated by the case histories reported in the book), the client being enabled to project his difficulties on to the social worker. The latter must then be able to point out fairly all the relevant factors of the situation *as they concern the client*, trying to strengthen his determination and power of decision, and not merely to persuade him against his will into what might appear at first sight to be the wisest course of action.

There is no doubt that an understanding of these considerations is of the greatest importance to anyone who intends to become a social worker, especially if this involves dealing with grants of relief. Such an understanding is essential in preventing what has sometimes unkindly been called the "C.O.S. attitude". At the same time this book is more valuable in raising this problem for consideration than in elucidating it fully, since its treatment of the subject is somewhat confused and repetitive. Nevertheless, the book provides a useful introduction to a question which is exceedingly interesting and important from both the theoretical and the practical points of view.

Conceptual Thinking in Schizophrenia. By Eugenia Hanfmann and Jacob Kasanin. New York: Nervous and Mental Disease Monographs, No. 67. 1942. Pp. vii+115. Price $2.50.

This is a valuable account of the use of Vigotsky's Concept formation Test, originally due to Ach and modified by Saharov, in an investigation of the degree of impairment of thinking in schizophrenia. Vigotsky had concluded that schizophrenics regressed from the conceptual thinking of normal people to a more primitive level, but Hanfmann and Kasanin have found it necessary to postulate three levels of thinking. They grouped their schizophrenic subjects and normal controls into those of superior and those of average education, and found that of the normals only the superior class formed concepts in the test. These also differed more from the other three classes than the latter did amongst themselves. However, it was possible to prove impairment in some schizophrenics and none in others, but there

was a third group in which it was unproven. These three groups corresponded to definite clinical pictures; e.g., no impairment was found in the group characterized by a prevalence of neurotic symptoms. On the whole, if schizophrenics were compared with normals of their own educational level, impairment was found. It would be of interest if the authors could find out exactly how performance of normals in the test is correlated with intelligence, and if possible what part emotional and other factors such as personal taste and prejudice play in hindering or assisting their performance. The method of scoring described by the authors is found to have many difficulties in practice by the reviewers. A. S. P., R. W. P.

Referred Pain; A New Hypothesis. By G. W. Theobald. The Times of Ceylon, Ltd. 1941. Pp. 40. 2*s.* 6*d.* net.

The author has made many clinical observations on referred pain especially on forms of uterine pain such as dysmenorrhoea, labour pains and the pain associated with the silver nitrate treatment of affections of the cervix. This evidence, together with some scanty experimental data, has prompted the hypothesis of a 'pain apperception centre' in the brain. Some of the speculation is interesting, but the evidence given in support of this hypothesis is unconvincing as all too often it is either vague or anecdotal.

From Thirty Years with Freud. By Theodor Reik. London: The Hogarth Press and the Institute of Psycho-analysis. 1942. Pp. 214. 12*s.* 6*d.* net.

It is difficult to see why in these days paper should have been expended on this unimpressive contribution to 'Freudiana'. It consists of a number of 'pious memories' of Freud, and of the effect he had on his students, which may be partly accurate but are certainly partly mythical, as the Editor himself points out. These are followed by some appreciations of Freud's later writings; and some not very interesting essays on certain minor Freudian matters. This is a disappointing book to have been produced by the author of *Surprise and the Psycho-analyst.*

Bishop Butler. By William J. Norton, Jnr. New Brunswick: Rutgers University Press. 1940. Pp. xi+336. $3.75.

Butler's theories of ethics, individual and social, have always been of interest to the psychologist because they claim to be based upon a study of fundamental propensities ('drives' as they might now be called) in human nature. Accordingly, this detailed study of his views will be welcome to many people. As a description, the book is excellent; as a criticism, in so far as legitimate criticism is possible in the light of the development of knowledge concerning the deepest springs of human conduct, it is less satisfactory. The author shows the fullest appreciation of the psychological character of Butler's views, but he seems to have little knowledge of modern discoveries which might well lead to a modification of these views at various points. Nevertheless, the book possesses great interest, for in several ways Bishop Butler anticipated recent psychological doctrines about the determination of human action. Particularly attractive is the study of Butler's views about society, views which, as the author rightly insists, are mainly built upon an observation of contemporary social organization. One is left wondering how Butler could have managed to fit the enormously increased complexity of modern social structure and functions into his comparatively simple frame.

EDITOR'S NOTE

In the paper by L. Litwinski entitled "Is there an instinct of possession?", in vol. xxxiii, Part 1, of this *Journal*, the term "attente" used in Section iii was, through an oversight on the part of the translator, translated "attention" instead of "expectation".